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A. P. WEISS

(1879-1931)

Albert Paul Weiss died on April 3, 1931. He influenced a great many persons by his studies and writing during the nineteen years he spent at Ohio State University as Professor of Experimental Psychology. His professional life may be regarded as covering the two decades in which American psychology has undergone significant change (1910-1930). He was one of the contributors to the shift in point of view, redefinition of subject-matter and problem, and extension of method which probably will be marked by future historians of psychology as characteristic of these years.

Professor Weiss was sensitive to problems; a skilled technician; an expert with apparatus; a keen, patient, and modest investigator. More than a score of his doctorates knew him and loved him for his fine qualities. Of course, the ultimate evaluation of any man's work must come with time. Those of us who lived and worked close to him knew the genuineness of his character, his geniality, his ardor for science. It is proposed here to record some of the more intimate, personal details of his life and work.

He was born of German Lutheran parents in Steingrund, Schlesien, Germany, September 15, 1879. In his early childhood the family came to America and settled in St. Louis, Missouri. The father, Joseph Weiss, was an architect, and possessed not only marked technical skill, but was also a man of scholarly interests, particularly in the field of philosophy.

Albert Weiss attended the public schools and then held a position with an engraving firm, attending art school at night. Not satisfied with this work, he entered the University of Missouri, supporting himself by doing university photographic work for various departments.

After first specializing in physics, mathematics, and philosophy, he turned to psychology as his major interest and became laboratory assistant to Professor Max F. Meyer in 1909. He was granted the A.B. degree in 1910 and the M.A. in 1912.

He went to Ohio State University as laboratory instructor with Professor G. F. Arps in 1912, returning to Missouri at intervals to continue the work begun with Dr. Meyer for his doctorate. The

dissertation *Apparatus and Experiments in Sound Intensity* appeared as *Psychol. Monog.,* 1916, 22, No. 5. The Ph.D. was granted in 1916.

In 1914 he was married to Miss Grace M. Parker, then a student at Missouri. His ability to carry on after he was stricken with an endocarditis in 1923 was in large measure due to the courage, sympathy, and loyal cooperation of his wife. Though doomed to an early death, he was cheerful, industrious, courageous, patient with a stoical fortitude to the end.

In 1918 he was given the title of Professor of Experimental Psychology, which he held until his death, which came suddenly and peacefully. Weiss, experimentalist and theorist, and Arps, experimentalist and administrator, cooperated in founding and developing the Ohio laboratory.

Professor Weiss held membership in a number of learned societies. Among these were Phi Beta Kappa, Sigma Xi, Phi Delta Kappa, and Alpha Psi Delta (psychological), A.A.A.S., Ohio Academy of Science, American Psychological Association (Member of the Council, 1929-1931) Midwestern Psychological Association (one of the founders and President, 1922-23). He was an associate editor of the *Journal of General Psychology* and contributed to *The Psychologies of 1925,* and also to *The Psychologies of 1930,* as well as to the Wittenberg symposium on the emotions. His writings include *A Theoretical Basis of Human Behavior,* R. G. Adams, Columbus, 1925; *The Behavior of the Newborn Infant,* Ohio State University Press Monograph, 1929; *Psychological Principles of Automotive Driving,* 1930; and about twenty-five original articles contributed to the journals on theoretical and experimental psychology.

He enjoyed many world-wide professional contacts. Yet few American psychologists have been held in higher or more general esteem than Weiss was held on his own campus, by the men in the other divisions of science. This is eloquent testimony, for it is exceptional.

I think it can be truthfully said of Professor Weiss's viewpoint that it was always held subject to modification as new evidence put in appearance. He was far less dogmatic, less intolerant of tradition in his teaching and in his seminar than some of his writing might lead one to presume. Weiss came into psychology with a grounding in physics, chemistry, biology, and mathematics. From his teacher, M. F. Meyer, he learned early to refuse to believe in the

"spectres of our tribal animism"—the faculties of mind, consciousness, will and the like, as satisfactory explanations of the cultural actions of man. While he regarded behaviorism as an ugly and unsatisfactory name, yet he accepted it for want of a better term. At a time when the unorthodox position was not only unpopular but precarious, Weiss hewed to the line as he saw it, fearlessly, enthusiastically, industriously, in presenting and developing his position.

Naturally, there were those who took sharp issue. The following is, I believe, a representative statement, made by a prominent past-president of the American Psychological Association: "While I did not agree with his general position, I always found him extremely tolerant and fair. His criticisms were keen and stimulating. I always enjoyed a discussion with Professor Weiss."

Weiss held that, in addition to "handling" responses, man has developed a system of signaling responses, language, which supplies the key to those phenomena of human conduct and achievement which hitherto had been attributed to non-physical forces. These signaling responses serve as stimuli to other individuals. The members of the speech-community can cooperate, by means of language, in spite of the biological limitation of discontinuity of the individual nervous systems. Viewed without its antecedents and consequences, *biophysically,* the signal (speech sound) is a slight displacement of matter; it is effective *biosocially* as a trigger-mechanism, thanks to the training (structural modification) of the speaker's and the hearer's nervous system. Thus the main problem of psychology is to describe "how the infant becomes the adult." The fundamental postulate is that all behavior of living things is a form of motion—of electrons and protons. These become organized after the fashion of well-known physical laws into atomic, molecular, crystalline, protoplasmic, unicellular, multicellular, and, finally, through language, the compound multicellular or social type of organization. Experimental psychology has as a chief province the description of the biosocial aspect of behavior. For Weiss there was no dichotomy between the physical and the social, nor was he willing to subscribe to the view that the biosocial category belonged to a classification of the emergent evolutionists. The methods which had sufficed the other divisions of science were those which should be adapted and extended in psychology. There are, of course, those who dissent from this view, namely, men like Thorstein Veblen and John Dewey, who writes "the existing limitations of social science are due mainly to unreasoning

devotion of physical science as a model, and to a misconception of physical science, at that" (*New Republic,* 1931, 47, 276). For Weiss psychology has as its distinctive province the *terra media* which overlaps biology and the social sciences, with the extension reaching farther afield into the social than the biological domain. For, "from a mere consideration of man's position in the scale of biological development, his libraries, museums, telephones, cinema, microscopes, telescopes, and skyscrapers, seem incomprehensible." A biophysical event is one stripped of all factors, dependent upon distinctively human response. Consequently, said he, it followed that little could be gained by a psychology whose problem was the hypothetical description of stimulus either as object-out-there or as the "experiencing person" or inner self which Titchener reduced to the physical experiencing nervous system (*Theoretical Basis,* pp. 244-245).

Professor Bloomfield writes "Weiss's style is direct. He says big things so simply that the reader, unforewarned, fails sometimes to grasp the full import. His plainness of speech sometimes goes beyond the conventional; a rhetorician would blue-pencil more than one of his sentences. Unhampered by ornament or mannerism, this Spartan style speaks powerfully and at times with great beauty. . . . In daily life his helpful competence, his utter divorcement from the simian and rapacious traits of our species, led one to see a high grace or saintly unselfishness in what for him must have been merely plain, sensible conduct. He attributed importance to no person, least of all to himself."[1]

At the time of his death his research energies were devoted to a series of studies on the behavior of newborn infants. This program he looked upon as the logical place to begin the experimental verification of his general theory.

Life for A. P. Weiss was a happy adventure, even under circumstances to try the most courageous of men. The spectre of death, close by him for years, never frightened him, or caused him to retreat from his devotion to the highest ideals of his science. He was a great teacher, a fine character. He had a part, a not insignificant part in the remaking of American psychology in the past two decades. His many biosocial signals still stand with us in the form of

[1]For the quotation from Professor Leonard Bloomfield, of Chicago University, the writer is indebted to Professor G. M. Bolling, editor of *Language,* in a forthcoming number of which Journal the article is to appear. The biographical facts were supplied by Mrs. Grace Parker Weiss.

his apparatus, lectures, and papers. His views of systematic psychology, whether they shall wax or wane, cannot be told in the near perspective. On all such matters Professor Weiss has expressed himself: "It seems to me that time is the most effective critic" (*Theoretical Basis,* vi).

SAMUEL RENSHAW

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THE GEOTROPIC CONDUCT OF ADULT RATS*

From the Laboratory of General Physiology and the Psychological Laboratory, Harvard University

MORGAN UPTON AND W. H. STAVSKY

I

Practically all the attempts that have been made to apply the theory of tropisms to the behavior of mammals have involved very young individuals. The behavior of the very young, unlike that of adult animals, is uncomplicated by the factor of extensive and highly developed potentialities for condition reaction; therefore less difficulty is encountered in the establishment of adequate control in an experimental situation. Crozier and Pincus (1, 2, 3, 4, 5, 9) have carried on extensive investigations upon the geotropic conduct of young rats, and have been able to present generalizations which are descriptive of the behavior of these animals under carefully controlled conditions. The mode of progression of an adult white rat on an inclined surface differs greatly from that of the young rat. The young rat creeps over the plane with the body in contact with the supporting surface, while the adult moves with its body free of the supporting surface, its weight being distributed to the four legs in a way determined by the position of the animal upon the plane and the postures of its legs. Inasmuch as these different modes of progression, as well as the different uses of the leg muscles, must involve different relative positions of the bodily musculature, it was desired to discover whether those laws found to hold for the behavior of *young rats* would hold also for the behavior of *adults*.

Hunter (8) has reported the results of an experiment with two blinded adult white rats. He concludes that the adult rat scatters its responses "more indifferently over a wide range of theta values than the young rats." He concludes further that the blinded adult animals creep at lower angles of orientation than do young rats, and states that he finds no significant relationship which could be mathematically stated between the values of θ and α; α is used convention-

*Accepted for publication by W. J. Crozier of the Editorial Board and received in the Editorial Office, June 23, 1931.

ally to denote the inclination of the plane upon which the animal creeps, and θ the angle between the base of the plane and the path followed by the animal in its progress over the surface of the plane. His findings regarding the geotropic behavior of young rats were such as to lead him to rather general disagreement with the conclusions of Crozier and his co-workers. The conditions which lead to this disagreement are not particularly pertinent in the present instance.

The tropistic interpretation of the behavior of young rats on inclined planes has been criticized by Hovey (7), who maintains that upward orientation is a learned response and in no sense a tropism. His conclusion is based upon data which seem to the writers to be totally irrelevant to the problem of orientation. In stating his problem he says, "If the reaction should increase in accuracy with repeated trials it would be necessary to conclude that memory and not a tropism is the determining variable in the response." To answer the question raised by this statement he placed an animal on a plane with the head toward the top of the boundary of the plane. Successive trials with many animals showed a decrease in time, such that, when plotted with logarithm of the time of a trial against the trial, a "learning" curve resulted. What possible significant relation this information can have to the precision of the orienting response the writers are unable to discover. It is to be expected that some adaptive change in the behavior of the animals would take place, and it might be expected that this would be indicated by less and less hesitation and more immediate activity as the experiment progressed and the animal became more accustomed to the situation. We fail to see where the data presented by Hovey had anything to do with a change in the precision of the orientation of the animals. In this problem we are concerned, not with the rate at which an animal adapts to the experimental situation, but with the angle of the path that the animal describes when it progresses up the plane. Hovey nowhere indicates that his results demonstrate an increasing precision of the values of θ as the experiment progressed.

Hovey states further that "the average values of θ are not important to an understanding of the behavior, for the reason that the averages do not represent the angles near which the greatest number of runs were made." Distribution curves of his data were so skewed as to be heavily loaded toward the vertical ($\theta=90°$). When plotted in terms of positive and negative θ's, with right-hand runs plotted

positively on the right and left-hand runs plotted negatively on the left of 90° as a midpoint, his curves, according to his statement, resemble average distribution curves with the highest mode near 90°. This is, in fact, true only for α=60°, 70°, and 80°, but for slopes of surfaces below α=60° the distribution curves are too flat for an approximation to a curve which fits the formula for the curve of average distribution. Under the conditions of Hovey's experiment he got just what might reasonably have been expected. The conditions differed from the conditions of the experiments done in Crozier's laboratory in several important details. The most important difference seems to be that Hovey's animals were placed upon the creeping surface with the head pointing upward, whereas the method ordinarily used has been to place an animal on the plane with the body-axis horizontal. With the body-axis vertical to the base of the plane the animal is perfectly oriented in the gravitational field, i.e., the muscular stresses on the two sides of the animal's body are necessarily equal. Under such conditions as those which obtained in Hovey's experiment, a grouping of θ-values in the region of 90° would be a reasonable prediction. With the body-axis vertical to the base of the plane there is no possibility of differential gravitational stimulation to bring about controlled orientation. From such a position any change in orientation must be conditioned upon purely chance factors. The muscle-tension theory of geotropic orientation assumes that an animal on an inclined plane will orient from a position in which the muscular stresses on the two sides of the body are *not* equivalent, until these stresses *are* equivalent. To test animals by placing them on an inclined plane with the head toward the top simply defeats any possibility of obtaining information pertinent to the problem, during the early portion of a trail. Further comment upon Hovey's paper may be found in a paper by Crozier and Pincus published in 1929 (5).

Our first attempts at experimentation with the adult rat were without success. In these attempts we employed the method ordinarily used with young rats. The experimental animal, utterly new to the situation, was placed upon the plane at various angles of inclination. At any particular angle the animal moved about the plane in practically any direction within the dimension of the plane. The movements were nearly always hesitant, slow, and in a curved path. Under such conditions it was impossible to collect constant data bearing upon our problem. If left on the plane, the animal eventually

reached one of the boundaries; in most cases contact with the boundary was made above the starting-point. In Hunter's report (8) of his experiments he states that the responses of his animals were predominately negative to gravity. Our early observations lead us to agree with him, but the trails were short, irregular, and of such a nature that it was impossible to be sure that they were true oriented geotropic responses. In short, the rat's behavior was in no sense orderly, and only seldom did the animal move in a straight line for more than six inches. We would conclude that under these conditions the activity of the adult rat displays no regular characteristics which could be regarded as indicative of any central tendency of response. Since we could be sure that we were dealing with oriented trails only when an animal moved in a straight line for a considerable distance, twelve inches was arbitrarily chosen as the shortest trail that would afford opportunity for accurate recording.

In these earlier experiments it was obvious that if the mechanism responsible for the geotropic responses in young rats was still present in the grown animals it was having little observable influence upon their behavior. This might have been due to the extreme multiplicity of stimuli (noises, handling by experimenter, etc.) which must necessarily affect the animal in the experimental situation. It would seem reasonable to assume that a grown rat, because of a heightened sensitivity to extensively developed conditioned reflexes, would be far more under the influence of what in the experiment were extraneous stimuli than a young animal, and that the total effect of these extraneous stimuli might presumably be prepotent over the geokinetic stimulation. In the case of the adult rat the gravitational forces acting upon it are apparently not strong enough to determine orientation; while for some reason they *are* prepotent in the case of the young rat.

II

Eight littermates, aged 14 weeks, of the King inbred albino strain, were used in the present experiment. Before being tested on the inclined plane, they were put through a training period which involved learning to go to the top of an inclined path 3 inches wide and 10 feet long, where they were fed 0.2 grams of dry food. This training was continued until all animals went directly, and without hesitation, to the top of the path when they were placed at the lower end. During the training period the inclination of the path was changed

so that all the animals had training at different inclinations ranging from very low angles to inclinations as great as 75 degrees. To get to the top of the path the animals obviously had to proceed along a trail of $\theta=90°$, since the narrow path prevented any deviation from this value.

After the training period, the rats were tested at various values of α on an inclined plane 84 cms. wide and 124 cms. long. To eliminate any disturbance from slipping, a heavy 3-mm. mesh screen was used as a creeping surface instead of the window screen which has been used in most experiments of this sort.

In carrying on the experiment an animal was placed upon the plane with the body-axis in a horizontal position, i.e., the body-axis was parallel to the base of the plane, and facing either right or left according to the haphazard choice of the experimenter. Coordinate lines were drawn upon the plane 15 cms. apart, and spots of luminous paint were placed at the intersections of the lines with the edges of the creeping surface. Records of trails described by the animals were made upon paper lined to represent the coordinates of the plane. The paper was illuminated from below by a very dim red light and all tests were made in complete darkness. A luminous marker held to the back of the animal by a strip of adhesive tape enabled the observer to follow the animal in its course over the surface of the plane. Ten trails were collected for each animal at each of several angles of inclination. On some days data upon the performance of the animals at two angles were collected, and all the observations were made within a period of eleven days. The room temperature varied from 19°C. to 23°C.

It was hoped that the training period would introduce a factor into the situation which would elicit something other than the "random" behavior that we found in our earlier experiment. Also, it seemed that light might be thrown upon the rôle which learning plays in geotropic behavior. If the animals simply learned to go *straight up* the path ($\theta=90°$) to get food, then, when they were placed upon a plane offering no restrictions to *lateral* movement, there would be no reason to expect any other type of behavior from them. On the other hand, if the animals had learned that food was to be had at the end of a run up an *incline,* it might be expected that the geotropic stimulation, if effective for the adult rat, would determine the angle at which an animal would ascend the plane. The results of the testing of the trained animals on a creeping surface at various angles of inclina-

tion, clearly demonstrated the correctness of the second hypothesis. The specific effect of the training was to make the animal negatively geotropic, but learning was not specific as to any given angle of ascent.

At no angle of inclination α other than 90° could the animals be said to go straight up the plane. When an animal was placed on the creeping surface the response was immediate and direct. From a position in which the body-axis was horizontal, the animal moved forward and simultaneously turned upward. At some value of θ determined by the slope of the plane, the turning ceased, and progression from that point onward was in a straight line until a boundary of the plane was reached. During the testing period each animal was always fed a small amount of food at the end of each trail. Very few downward trails were made, and in most cases these were made by animals which had eaten many times, and were, presumably, no longer hungry. When these downward trails were made, they always fell within the range of θ values for upward trails at the same inclination of the plane. That an animal orients downward under certain conditions in no way vitiates the tropistic interpretation of orientation in a gravitational field (5, p. 59-60). Behavior conditioned upon a reversal of tropistic sign is common enough, and any number of factors might account for a reversal here. It is obvious that a decrease in hunger, together with other unpredictable changes in the animal, might bring about a reversal of sign. In other cases, handling by the experimenter could conceivably cause a reversal of tropistic sign (6). At present an experiment is under way with another litter of adult rats which has as its aim the training of the animals to seek food at the end of a downward trail. From present indications, it appears to be much more difficult to train rats to go *down* an incline for food than it is to train them to go upward.

III

The data collected from observation of 8 animals and presented in Table 1 represents the values of mean angles of orientation θ at several angles of inclination α of the creeping surface. Each value of θ is the mean of 80 observations. Inspection of the table reveals that values of mean θ increase as the slope of the plane increases.

It is apparent from Figure 1 that there exists some functional connection between the angle of orientation θ, and the slope of the creeping surface α. As in the case of young rats (1, 2, 3, 4, 5, 9) the relation appears to be basically sigmoid in character when θ is plotted

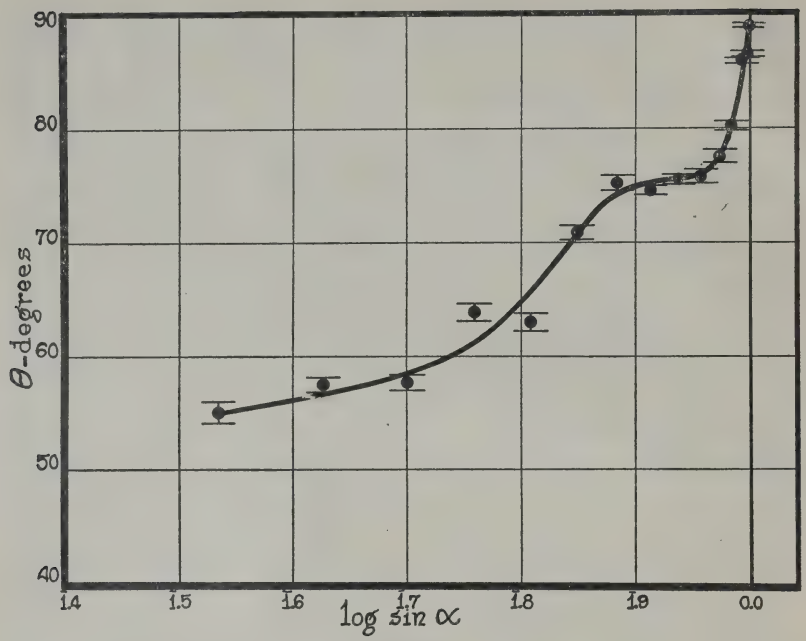

FIGURE 1

Mean angles of orientation for eight adult rats (littermates). The compound character of the relation between θ and α is apparent in this curve. The height of the vertical bars = 2 *P.E.* of the mean.

against log sin α. There also appears in this curve a striking similarity to the same type of curve derived from observations upon the geotropic behavior of young guinea pigs (10). In both cases the curves (θ *vs.* log sin α) are sigmoid in character, but, in addition, both curves are obviously compound, being made up of two sigmoid sections. With the guinea pigs, the junction of these sections occurred in the region α=45°—, while with these adult rats it occurred at a somewhat higher slope, in the region α=55°. With both young guinea pigs and adult rats it was observed that within the range of values of α corresponding to the break in the plotted function, the mode of progression changed from walking to a hopping movement in which both back legs, and alternately both front legs, are extended simultaneously. A different distribution of muscular tensions must accompany this change in mode of progression, and should thus determine a change in the relation of θ to α.

The essential similarity of the phenomena in the cases of both adult and young rats, young guinea pigs, and other mammals studied is clearly demonstrated when cos θ is plotted against sin α (Figure 2). The relation in all cases studied is essentially rectilinear with

TABLE 1

This table summarizes the mean angles of orientation (θ) for a group composed of eight albino rats of the King inbred strain. The animals were littermates, aged 14 weeks, and the entire experiment was completed within a period of eleven days. Eighty observations are averaged for each value of θ.

Slope of surface α degrees	Angle of upward orientation θ degrees	P.E. θ degrees
20	55.1	±0.85
25	57.5	±0.54
30	57.7	±0.69
35	63.9	±0.53
40	63.0	±0.65
45	70.9	±0.45
50	75.3	±0.53
55	74.6	±0.32
60	75.4	±0.35
65	75.8	±0.56
70	77.6	±0.51
75	80.3	±0.22
80	86.0	±0.25
85	86.6	±0.20
90	89.0	±0.11

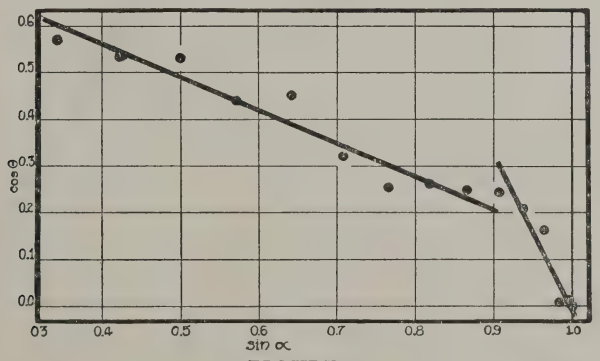

FIGURE 2

Cos θ as a function of sin α. The two straight lines represent regions in which the proportionality factor differs.

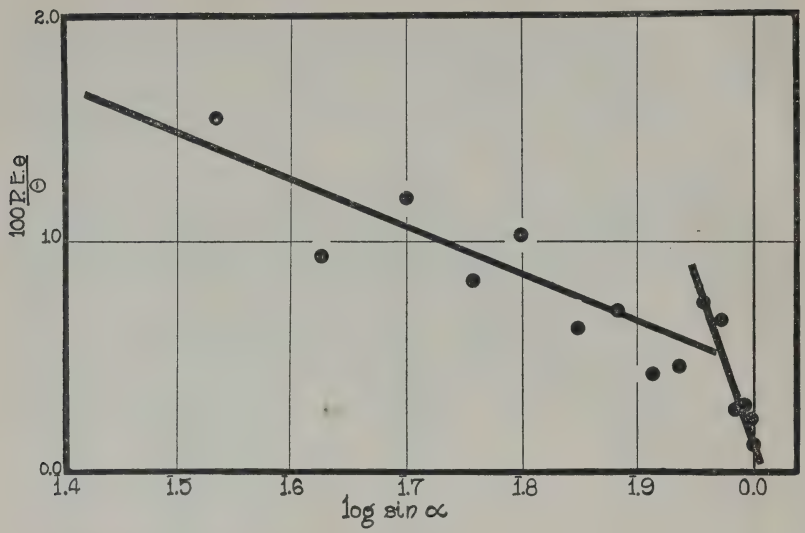

FIGURE 3

Relative variability of mean θ plotted against log sin α. The break in the slope of relationship occurs at approximately $\alpha = 55°$.

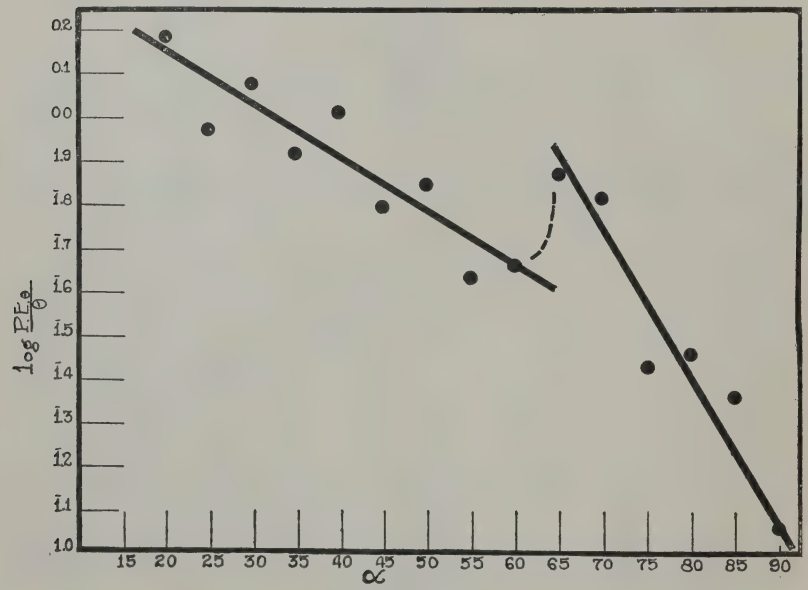

FIGURE 4

The discontinuity is much more apparent when log ($P.E._{\theta}/\theta$) is plotted against α.

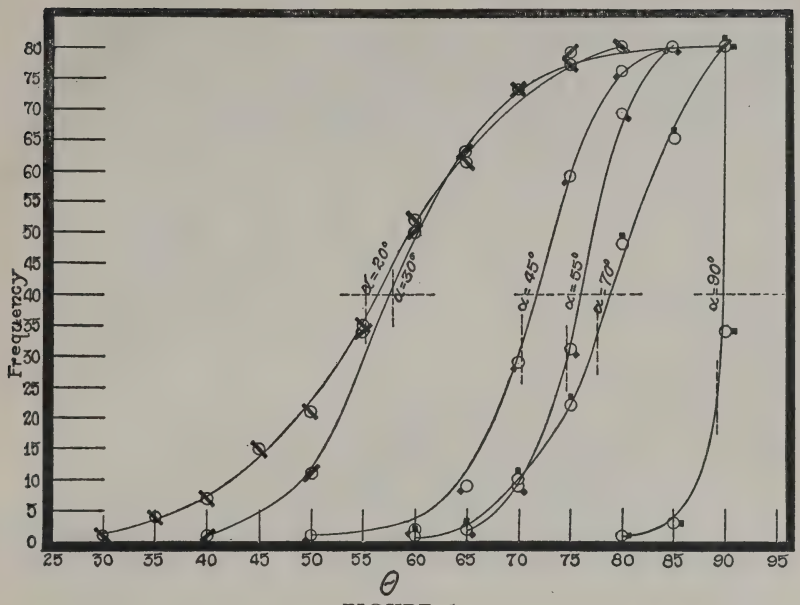

FIGURE 5

Summated frequency distributions for values of θ at α=20°, 30°, 45°, 55°, 70°, and 90°.

cos θ decreasing as sin α increases. With the adult rats and young guinea pigs (10) there are two regions representing different factors of proportionality operative at high and low slopes respectively.

In Figure 3 it is clear that the relative variation of mean θ declines rectilinearly with an increase in log sin α. Here in the variability plot we find again an apparent zone of discontinuity which appears in the region of α=55°, just as it did in Figure 1. This would seem to indicate that the response and the variation which characterize it are, in some way, yet to be determined, controlled by the same set of conditions. It is certain that it does indicate, as Crozier (5, pp. 70 ff.) has pointed out, that the variability of the observed response is not capricious but is, in fact, lawful. The break in the variability plot is much more obvious if we plot upon the same coordinates log $P.E._{\theta}/\theta$ against α (Figure 4). Again the relationship is apparently rectilinear but discontinuous, with a break in the region of α=55°.

The frequency distributions for several angles of inclination α

(Figure 5), indicate that there was no definite grouping of θ values around 90°. The distributions decrease in span as the slope of the plane increases and the means are in all cases located near the median and the inflection points of the summated frequency curves. There is a degree of negative skewness due to the fact that 90° is the limiting value of response; this negative skewness is, however, not without theoretical significance (5, p. 68).

Summary

Untrained adult white rats tested on an inclined surface display "random" behavior of a type which affords no information pertinent to the problem of orientation in a gravitational field. However, when animals which have been trained to ascend a narrow path at various angles of inclination are tested on an inclined surface, their behavior obeys the same general laws which have been established for the behavior of young rats and guinea pigs.

The contention that orientation in a gravitational field is the result of a conditioned response appears to be untenable, since specific training breaks down when the trained animals are tested on a surface which removes the constraint imposed by a narrow path. Instead of ascending the plane along a trail vertical to the base of the plane and corresponding to the "learned" path, the animals ascend at an angle θ which is a definite function of the inclination α of the plane.

The training apparently makes the animal negatively geotropic, and this state under the conditions of the experiment becomes the determining factor in the specific behavior on an inclined surface. Those potentialities in the organism which determine the tropistic orientation become prepotent, not only over the extraneous stimulation which determines the "random" behavior observed in the untrained animal, but also over the specific conditioned reaction of ascent in a path vertical to the base of the plane.

The relation of θ to log sin α is sigmoid in character, but the function is in reality compound, being made up of two curves with the junction occurring in the region of $\alpha=55°$. This was also true in the case of the young guinea pigs, but the junction was there at a lower slope, near $\alpha=45°$. In both cases the break in the plotted functions appears to be correlated with a change in mode of progression from walking to a hopping movement in which both hind feet and alternately both front feet are extended simultaneously.

This changed mode of progression is thought to account for the change in the relation of θ to α. The discontinuity occurs in a corresponding region in the graph obtained when the relative variation of mean θ is plotted against log sin α, thus indicating a single set of determining factors for both the mean response and its variation.

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LE COMPORTEMENT GÉOTROPIQUE DES RATS ADULTES

(Résumé)

Des rats blancs adultes non entraînés testés sur une surface inclinée montrent un comportement 'au hasard' d'un type qui ne donne pas de renseignements applicables au problème de l'orientation dans un champ de gravitation. Cependant, quand des animaux entraînés à monter sur un sentier étroit à divers angles d'inclination son testés sur une surface inclinée, leur comportement obéit aux mêmes lois générales établies pour le comportement des jeunes rats et des cobayes.

L'assertion que l'orientation dans un champ de gravitation est le résultat d'une réponse conditionnelle paraît insoutenable, puisque l'entraînement spécifique ne réussit pas quand les animaux entraînés sont testés sur une surface qui enlève la contrainte imposée par un sentier étroit. Au lieu de monter sur le plan par un sentier vertical à la base du plan et correspon-

dant au sentier "appris," les animaux montent à un angle θ lequel est une fonction définie de l'inclination α du plan.

Il paraît que l'entraînement rend l'animal négativement géotropique, et cet état dans les conditions de l'expérience devient le facteur déterminant dans le comportement spécifique sur une surface inclinée. Les qualités potentielles dans l'organisme lesquelles déterminent l'orientation tropique deviennent puissantes, non seulement sur la stimulation externe qui détermine le comportement 'au hasard' observé chez l'animal non entraîné, mais aussi sur la réaction conditionelle spécific de l'ascention par un sentier vertical à la base du plan.

La relation de θ au log sin α est de caractère sigmoide, mais la fonction est en réalité composée, formée de deux courbes avec la jonction dans la région de $\alpha = 55°$. Cela a été vrai aussi au cas des jeunes cobayes, mais la jonction a été moins élevée, près de $\alpha = 45°$. Dans les deux cas l'interruption dans les fonctions tracées paraît corrélèe avec un changement de la méthod de progression d'un mouvement de marcher à un mouvement de sautiller où les deux pattes de derrière et alternativement les deux pattes de devant sont étendues simultanément. Cette méthode changée de progression expliquerait le changement dans la relation de θ à α. La discontinuité se trouve dans une région correspondante du graphique obtenu quand la variation relative du moyen θ est tracée contre log sin α, indiquant ainsi un seul groupe de facteurs déterminants pour la réponse moyenne et sa variation.

UPTON ET STAVSKY

DIE GEOTROPISCHE TÄTIGKEIT ERWACHSENER RATTEN

(Referat)

Ungeübte Ratten, die auf einer schrägen Fläche eingeübt werden, benehmen sich auf's Geratewohl, auf eine Weise, die über die Aufgabe der Orientierung in einem Gravitationsfeld keine Belehrung erteilt. Wenn man jedoch Tiere, die dressiert worden sind, einen engen Pfad bei verschiedenen Inklinationswinkeln (angles of inclination) zu ersteigen, an einer schiefen Ebene prüft, so folgt ihre Tätigkeit den selben allgemeinen Gesetzen, die in Bezug auf die Tätigkeit junger Ratten und Meerschweinchen festgestellt worden sind.

Die Behauptung, die Orientierung in einem Gravitationsfelde sei die Auswirkung einer bedingten Reaktion, scheint unhaltbar zu sein, da die spezifische Dressierung zusammenbricht wen die dressierten Tiere auf einer Ebene geprüft werden, welche die durch einen engen Pfad auferlegte Einzwangung beseitigt. Statt die Ebene auf einem Pfad zu ersteigen, der zu der Grundfläche der Ebene senkrecht steht und der dem "erlernten" Weg entspricht, steigen die Tiere auf einem Winkel, der eine bestimmte Funktion der Ebene darstellt.

Die Dressierung macht die Tiere scheinbar negativ geotropisch, und unter den Umständen des Experimentes stellt dieser Zustand das bestimmende Element bei der spezifischen Tätigkeit auf einer schiefen Ebene dar. Jene Potentialitäten des Organismus die die tropische Orientierung bestimmen werden vorherrschend, nicht bloss über die unwesentlichen Reizungen, die die beim undressierten Tiere beobachtete zufällige Tätigkeit bestimmen, sondern auch über die spezifisch bedingte Reaktion des Ersteigens auf einem der Grundfläche der Ebene senkrechten Pfad.

Die Beziehung von θ zum log sin α ist sigmaförmiger Art (sigmoid). Die Funktion ist aber in Wirklichkeit eine zusammengesetzte, bestehend aus zwei Kurven, deren Vereinigungspunkt in der Umgebung von $\alpha = 55°$ liegt. Dies war bei den jungen Meerschweinchen auch der Fall. Heir lag aber der Vereinigungspunkt auf einer niedrigeren Schräge, ungefähr bei $\alpha = 45°$. In beiden Fällen scheint der Bruch in den graphisch dargestellten (plotted) Funktionen in Beziehung zu stehen zu einer Veränderung in der Weise des Ganges (mode of progression). Aus dem Gehen wurde eine Hüpfen, wobei beide Hinterfüsse und, abwechselnd, beide Vorderfüsse zu gleicher Zeit augestreckt wurden. Dieser veränderte Gang wird wohl die Veränderung in der Beziehung zwischen θ und α erklären. Die Unterbrechung findet sich in einem entsprechenden Teil des graphischen Bildes (graph) das erhalten wird wenn man die relative Variation des mittleren θ gegen log sin α graphisch darstellt (plots). Hierdurch wird angedeutet, dass bei der mittleren Reaktion und bei ihrer Variation eine einzige Gruppe von bestimmend en Funktionen wirksam ist.

UPTON UND STAVSKY

DRIVE AND REFLEX STRENGTH*

From the Psychological Laboratory, Harvard University

B. F. Skinner

I

We say that an animal is hungry if, when we give it food, it eats. We also say it is hungry if it responds in characteristic ways to an empty food dish, to the approach of the experimenter, or, in short, to any stimulus that has been conditioned to behavior in some way concerned with eating. In the absence of such specific stimulation, we may say that the animal is hungry if it shows what has unfortunately been called spontaneous activity, although we cannot in such a case distinguish between hunger and some other drive until a stimulus leading to a specific response has been introduced. In our everyday use of the word only the first of these conditions is important, for we ordinarily attribute hunger to an animal only because it eats or because it exhibits behavior that we have frequently observed to be followed by eating. In the latter case the use of the word rests upon the conditioning of the experimenter.

When we use the term hunger, therefore, we are describing the presence or absence, the display or want of display, of certain characteristic behavior. Thus we are concerned with the fact that an animal eats, or that it does not; that it approaches an empty food dish, or that it does not; that it runs in a running wheel, or that it does not; and so on. The question of hunger arises only because the behavior of an organism may exhibit alternatives of this sort.

The central problem in the study of hunger, then, is to account for the appearance or non-appearance of a given sort of behavior at a given time. To this end we may try to show that the appearance of a given piece of behavior is a function of some designated variable, for example, of time. The conditions of an experiment to determine the nature of that function will be fairly simple. We have only to place an animal in a familiar cage with a supply of food, eliminating,

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so far as possible, all incidental stimuli, and to observe the times at which it eats. Such an experiment was performed, as is well known, by Richter (2), who found that over periods of time of the order of magnitude of days a white rat shows an orderly periodicity in its eating activity.

But the simple observation of whether a rat eats is, after all, only an all-or-none measure, while we may (and almost always do) treat hunger as a matter of degree. If we place a very hungry rat with a supply of food in a controlled situation, it will eat for perhaps two hours and then stop. In such an instance we should say that the hunger of the animal has undergone a marked change in degree and has eventually disappeared. An adequate description of hunger must account for a change of this sort.

In such a case what have we actually observed? We are likely to have noted first of all that the speed of each component part of the rat's behavior gradually slackened, and that the various parts of a coordinated movement followed each other less and less quickly. Another change may have been evident in the length of the interval between the eating of successive pieces. At first this interval was probably scarcely observable, but it became more and more marked and may have reached the length of four or five minutes before the rat stopped eating altogether. We may also have observed that, whereas at the beginning of the period the rat came immediately to the food, it did not (if we removed and returned the food) do so as promptly later on. Further, we may have observed that it became less and less difficult to inhibit the eating behavior with extraneous stimuli.

The experimental isolation of any one of these changes is difficult and in some cases apparently impossible. A combined measure of several is available, however, in the *rate* at which the rat eats. This rate is obviously determined by some of the factors we have listed: the speed of the component movements, the interval between the eating of successive pieces, and so on. As a first experimental step, then, we may examine this rate as a function of time from the beginning of an eating period.

II

A standard food is prepared by pressing a stiff mash (McCollum's formula) into strings, which are then dried and broken into pellets weighing 15 (in some cases 20) to the gram. (Recently a machine

built on the model of a druggist's pill-machine has been used, which yields pellets of a more uniform shape). By preparing the food in this way one reduces the rate of eating to the rate at which these standard pieces are taken up by the rat, and this may be recorded mechanically with the aid of the following devices.

A food tray is constructed as shown in Figure 1*A*. The rat stands on the platform at the left and obtains food from the pocket at the right. The food is placed below the level of the platform so that the rat must withdraw from the tray before eating. In obtaining food the rat pushes inward the light door that hangs in the opening to the food pocket. The door is counter-balanced and moves with great ease. When the door is opened, a contact is made at *A*, and these contacts are recorded as described below. A screen not shown in the drawing shields the flash of the contact from the animal. A small bellows, the use of which is described later, is shown at *B*. The drawing is not exactly to scale, and the door is free to open farther than is here indicated. The device is about 10 inches high. In the model used, two of these pockets stand at the end of a common platform, one of which contains water.

Unfortunately, such a device is not entirely adequate. A rat will frequently "rattle" the door in taking a piece of food, thereby making more than one contact. To eliminate this effect a special device is introduced into the circuit to the recording lever, as shown schematically in Figure 1*B*. A wooden disc (*A*), 6″ in diameter and weighing about a pound, is suspended on four strings, 18″ long, fastened near the center of the disc. A pin is driven into the base through a hole in the center of the disc, which revolves freely about it. The disc is twisted several times in a clockwise direction until it tends to turn in the opposite direction with some slight force. A small arm on the disc is then arranged to come to rest against the soft iron armature of a coil magnet (*B*). The latter kicks the disc in a clockwise direction when the magnet is activated. The disc is adjusted to make nearly a full revolution; it then turns back and comes to rest against the armature.

The circuit from the food tray coming in at the lower right-hand corner of the drawing goes directly to the magnet (*B*) but to the recording lever only through a second contact at *C*. When the rat opens the door of the tray, both the magnet (*B*) and the recording lever are activated. But the circuit to the recording lever is immediately broken at *C* by the action of the disc and remains broken

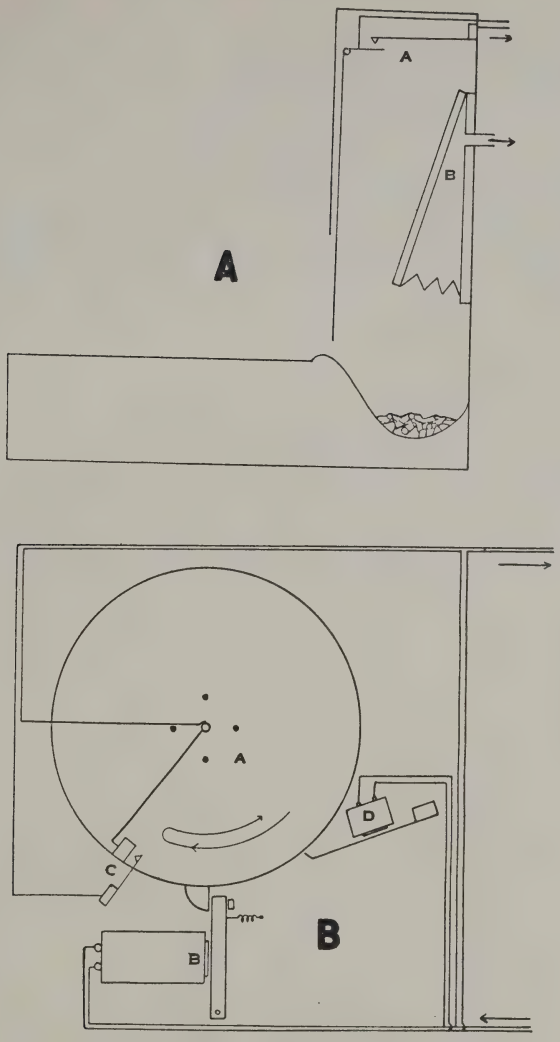

FIGURE 1

A. Longitudinal section of food tray, showing contact (*A*) and small bellows (*B*).

B. Plan of device to eliminate superfluous contacts. See text.

until the disc has returned to rest. Superfluous contacts are thus kept from the recording device, and by altering the weight of the disc a suitable interval may be obtained that will restore the circuit for the next legitimate contact. Additional protection against a premature return of the disc is afforded by another coil magnet at *D* which holds a light lever against the disc to prevent its return while the contact in the food tray is held closed.

An experimental box, 55 cm. long, 50 cm. wide, and 20 cm. deep, built of double walls of "Celotex" and mounted upon rubber blocks, is inclosed in another "Celotex" box, also mounted upon rubber. Both boxes are placed in a semi-soundproof experimental room. The experimental box is dark throughout the experimentation and is continuously ventilated with a slow stream of washed air pumped through a small tube from an adjoining room. Most of these precautions have been found necessary for the success of an experiment. The food and water trays are set inside and against one wall of the experimental box. The recording instruments are in an adjoining room.

Adult albino rats of the King inbred strain, of the 65th and 66th generations of brother-sister inbreeding, have been used. At the same hour daily a rat is taken from the animal room and placed in the experimental box with an ample supply of food and water. The box and the room are then darkened and closed for the duration of the experiment. Ordinarily the rat is left in the box until at least 30 minutes have elapsed during which no eating has taken place, as shown on the records in the adjoining room. The rat is then removed from the box, returned to the animal room, and given about five grams of surface-dry lettuce leaf. No other food is given to it.

In recording the contacts a simple signal lever writing upon a kymograph drum would suffice, but we are interested in *rate* and in changes in rate, and a slight modification in the method of recording simplifies the inspection and measurement of these aspects of the record. A writing point is mounted upon a small collar, which slides freely upon a vertical shaft. The collar is supported by a silk thread, which is wound about the shaft of a ratchet mounted at the top of the vertical shaft. The ratchet is turned one notch at each contact by a coil magnet. Thus, when the circuit to the coil is made, the ratchet turns one notch, the thread is wound slightly further about the shaft of the ratchet, and the writing point is drawn up a corresponding (and uniform) distance. The record of a series

of contacts is a step-like line traced diagonally upon the kymograph paper. The slope of the line varies with the frequency of the contacts, and the line is straight, of course, if the rate is constant. The record may be treated as a graph, and in the figures reproduced here suitable coordinates have been added.

In Figure 2 a typical record obtained with this procedure has been reproduced directly from the kymograph tracing. It will be seen from the curve that the rate of eating varies in an orderly fashion, beginning at a maximum and decreasing regularly throughout the period. It has been pointed out in an earlier report (3) that the typical curve closely approximates a parabola, and experiments made after an interval of a year, with a later generation of the same strain of rats, fully confirm that statement. The equation for the curve is

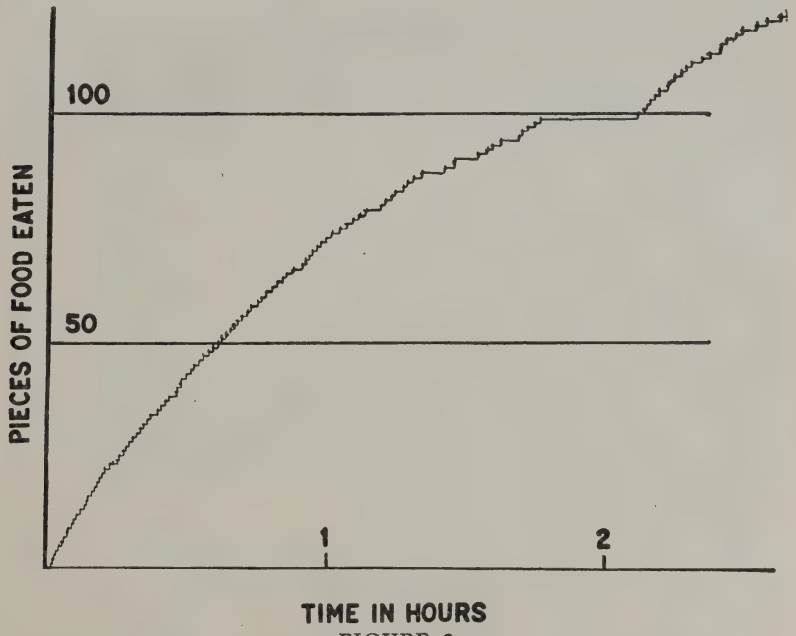

FIGURE 2

Kymograph record of the eating behavior of a white rat. At each elevation of the writing point the rat ate a pellet of food of standard size. The rate of eating is given by the slope of the line and can be seen to decrease regularly throughout the period of 2½ hours. The curve is described by the equation $N=Kt^n$, where N is the number of pieces eaten at time, t, and K and n are constants.

$$N = Kt^n$$

where N = amount of food eaten at time, t, counted from the beginning of the period, and K and n are constants. The magnitude of n (the important constant in determining the nature of the curve) was given originally as 0.67 to 0.70. Recent determinations, where several parts of the earlier procedure have been modified, place it at approximately 0.70.

Figure 2 is not an exceptional record. Curves of equal regularity may be obtained daily, if the experiments are conducted with due care. There are, of course, minor deviations from a smooth curve. These are attributable to several factors. There is, first, the failure of the recording apparatus to follow the behavior of the animal, especially when this is atypical, as, for example, when the rat takes a piece of food from the tray, drops it, does not recover it, and returns again to the tray. In such a case two pieces will be recorded for one eaten. Accidents of this sort are fortunately infrequent. Secondly, there are irregularities in the behavior of the animal, which are correctly reported in the records, and which we must note in more detail.

It will be seen in Figure 2 that near the end of the second hour the rat stops eating, to begin again only after an interval of some 20 minutes. At the end of the interval the rat is, so to speak, considerably behind the schedule set for it by the earlier part of the curve. When it begins again, however, it eats at a greater rate, so that within half an hour the record has reached the extrapolation of the body of the curve. The rate is not constant during the period of recovery but falls off in much the same way as in the original curve. Following such an interval, the record has never been observed to go beyond the extrapolation of the main curve and probably falls slightly short of it in most cases.

Other minor examples of the same phenomenon may be discovered in Figure 2. A record is seldom obtained without them. The interruptions themselves may be accounted for in several ways. During some of them the rat drinks. This had been determined with a signal device attached to the water tray. Other delays may be attributed to urination and defaecation. The longer intervals in the latter part of the records cannot be so explained, and must be due to conflicting behavior of some other sort. The compensating increase following an interval raises a more difficult question and

one that is perhaps impossible to answer without postulating a mechanism to account for the curve itself. No hypothesis will be advanced here, but a few experiments that bear upon the process of recovery may be described.

A small bellows, as shown in Figure 1*A*, is arranged to press outward against the door of the food tray when inflated. The bellows is controlled from a source of compressed air in the adjoining room and permits the experimenter to lock the door to the tray without otherwise disturbing the rat. Intervals of any desired length may thus be introduced into a record, during which the rat cannot eat. In the following experiments the intervals are approximately 10 minutes long and are begun as soon as the general trend of the curve has been established or when about the thirtieth piece of food has been eaten.

Figure 3 shows a typical record obtained when the pneumatic lock

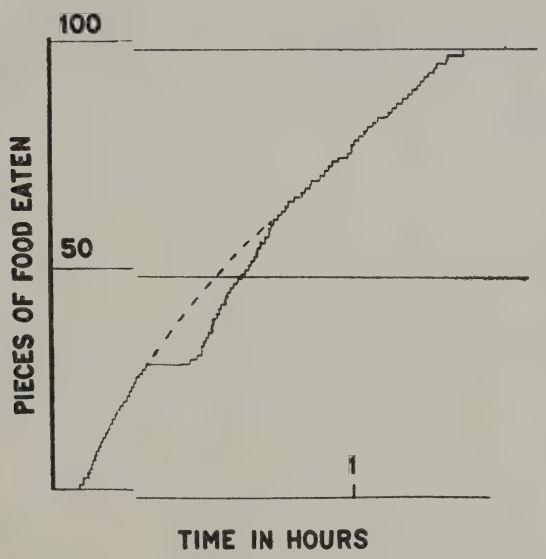

FIGURE 3

Kymograph record of the eating behavior of a rat, where the supply of food is cut off for ten minutes. The rat "recovers" from the interruption at an increased rate, so that the record reaches the extrapolation of the first part of the curve within twenty minutes. (The record has been cut and shifted vertically to correct for two contacts made accidentally by the experimenter.)

is used for the first time. The curve is interrupted, of course, and following the enforced interval the rat eats at an augmented rate. The extrapolation of the original curve is reached within 20 minutes. The recovery curve, however, differs significantly from that of Figure 2, as may be most clearly seen by foreshortening the records and sighting along the curves. In Figure 3 the recovery takes place at an approximately constant rate and the record shows a break where the original curve is reached. In Figure 2, on the other hand, the recovery shows a progressively diminishing rate and the original curve is approached asymptotically.

When the lock has been used several times, the curves begin to assume the character of the "normal" recovery. Enforced intervals had been introduced on two previous occasions when the record in Figure 4 was taken. Although a break is still discernible where the original curve is reached and the recovery is relatively slow, the curve has become convex and obviously similar to the normal recovery curve. An explanation of this change must, here again, presuppose some hypothesis for the curve itself. For the present we

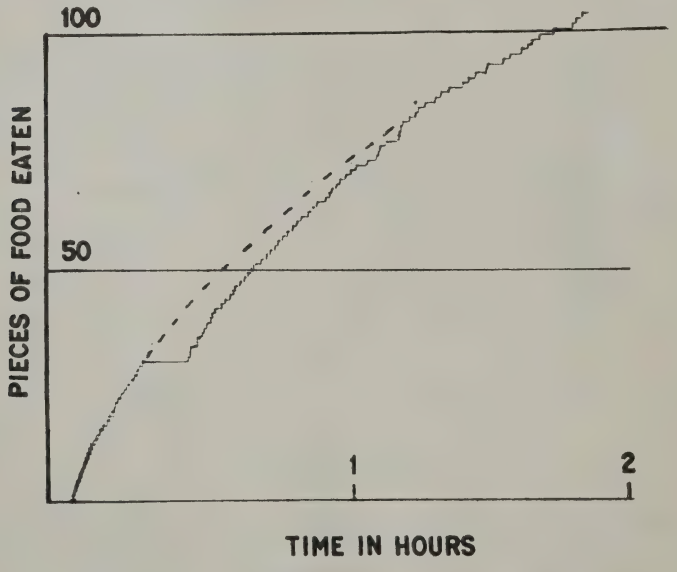

FIGURE 4

Record as in Figure 3, but upon the third occasion when the supply of food is cut off. For discussion see text.

may note simply that the "straight-line" recovery of Figure 3 is probably the resultant of a normal recovery and an emotional effect set up by the exclusion from the food tray. On subsequent occasions the emotional effect may adapt out, leaving the normal recovery curve. The possibility of determining the course of such an emotional effect through the analysis of records of this sort and the broader question of what is meant by an emotion in such a case are reserved for future discussion.

III

The problem of hunger arises, as we have seen, because the behavior of the intact organism shows a certain apparent variability. We have discussed a problem of this sort elsewhere, in an examination of the reflex as a concept in the description of behavior (4). There we saw in part that the description of behavior is concerned with at least two sorts of law. The first of these deals with specific uniform relationships between behavior and the forces acting upon an organism. A reflex is itself a law of this type, which describes the observed correlation of two events, a stimulus and a response. Supplementary laws (of threshold, latency, and so on) describe more exactly the conditions of such a correlation. The laws of the second sort, however, have no reference to the single elicitation of a reflex but describe changes observed during successive elicitations. Such are the laws of reflex fatigue, the refractory phase, summation, and so on. We left that discussion with the assertion that drive, emotion, and conditioning, so far as they concerned behavior, could be described with laws of the second sort. Let us see more exactly what this means, at least so far as it relates to the subject of drive.

The behavior of a rat in approaching, seizing, and eating a piece of food is, by assumption, reflex, or more accurately it is composed of a chain of reflexes, which are closely interwoven although they may still be specified and observed individually. In the latter part of the series the chaining is effected chiefly by the movement of the bolus of food, in the earlier by the movement of the organism in space; but in both cases the rule is the same: that the response to one stimulus brings up the stimulus for a subsequent response. In the initial members of the chain certain visual, tactual, and olfactory stimuli initiate movements of orientation, first of the head, then of the body, followed in turn by movements of progression. New stimuli are supplied internally and externally by every part of this

activity, and the lips of the animal are eventually brought into contact with food. Thereupon, seizing and chewing reflexes (studied, for example, by Magnus in the thalamic rabbit (1), and the usual swallowing reflexes, carry the food to the stomach, where it passes out of range of the present discussion.

But what is our justification for regarding the initial members of this chain as reflex in nature? *If it is in fact true that a rat's approach to a bit of food is reflex, why is the response not always evoked by the appropriate stimulus?* This is a convenient question. It will be recognized at once as typical of a very widespread criticism of the concept of the reflex. At the same time it epitomizes the present problem.

The inevitability of any reflex, the necessity of the relationship of any stimulus and response, rests ultimately upon observation: a response is observed to follow the administration of a stimulus and to be absent otherwise. But no reflex shows an absolute necessity of this sort. For example, the necessity is lacking during the absolute refractory phase and after complete fatigue. Necessity, moreover, implies a constant ratio of the values of stimulus and response, which is, nevertheless, seldom observed. No one will be likely to urge these exceptions against the validity of a reflex, for the conditions under which they are observed are almost always induced experimentally, with the result that the experimenter is provided with some means of accounting for an effect even before he has observed it. But such an appeal to a third variable (to a condition of the experiment, for example) will be fully satisfying only if the effect can be shown to be an exact function of the variable, and we ordinarily attempt to secure this satisfaction by demonstrating the nature of the function—for example, by discovering the "curve" for the refractory phase or for reflex fatigue. In this way we effectually eliminate a so-called variability by describing it. The same procedure is available in dealing with the problem of drive.

Before interpreting our present results, however, it will be necessary to mention a matter of terminology. Such a characteristic as reflex fatigue, as we have noted elsewhere, involves extensive changes in latency, threshold, after-discharge, and so on. Nevertheless, we ordinarily regard it as a unitary process. It is customary, for example, to measure only one of its aspects (for example, the change in after-discharge) and to assume that a variation observed there will exemplify the total process. But if we are asked *what* it is that

changes during reflex fatigue, we can answer only by detailing the specific factors of latency, magnitude of response, threshold, and so on. We lack a term to stand for the totality that undergoes the change. The physiologist thinking in terms of the synapse can speak conveniently of an increase in synaptic resistance or the exhaustion of a synaptic substance or state, and may attribute all the observed effects to this one change. If we wish to remain at the level of our observations, however, we must have a term that serves the same purpose without committing us to a physiological theory. We have suggested (4) the term "reflex strength," which is not new, although it has never been used in any very exact sense. The strength of a reflex is given by the value of its threshold, the ratio of the values of its stimulus and response, the duration of its latency, the amount of its after-discharge, and so on. It is thus a convenient term in describing a process that involves all these factors.

The instances of variability that raise the question of hunger are expressible in terms of reflex strength. The term accounts for the aspects of the behavior that indicate degree of hunger—which we may equally well speak of as characteristics of the component reflexes—and with the aid of the principle of a threshold it also accounts for the mere appearance or non-appearance of a response. Thus, to say that an animal is very hungry is only to say that certain reflexes concerned with the ingestion of food are strong. To say that an animal is not hungry is to say that the strength is below a threshold point. And so on. The theoretical value of this interpretation, beyond its present application, need not be discussed here.

The problem of hunger presents itself, then, as a variation in the strength of certain reflexes, a variation which ordinarily appears haphazard. The solution of the problem lies in the description of the variation. In the present experiment, accordingly, if we take the rate of eating to be a measure of the strength of these reflexes (an assumption that is reasonable in view of what we have already said), we find that the strength is an exact function of the time from the beginning of an eating period. This is demonstrated, of course, for only one from among an indefinite number of cases (corresponding to the number of possible experimental situations). But the instance is typical, and it seems a reasonable assumption that under other conditions the strength is also similarly determined.

What we have said for hunger holds equally well for other drives,

due allowance being made for a difference in the type of behavior involved. If we define any given drive wholly in terms of our observations, we shall find that it is a matter of whether or not a response is elicitable, or of certain quantitative aspects of an elicited response. In either event it is expressible in terms of reflex strength. Consequently, the experimental treatment of drive will take the form we have indicated: the examination of some measured aspect of strength as a function of some such variable as time.

It may be objected that in this discussion we have somehow omitted the heart of the problem. We have had little or nothing to say about a *something* called drive. Is this a real omission? The first form that this objection is likely to take is that we have failed to touch upon the physiological side of the subject. Drive, it may be argued, is, after all, a reference to a physiological state. Must we not seek for a definition in physiological terms? Now, that there are physiological conditions correlated with all aspects of behavior no one will be likely to question. But it is equally true that behavior may be adequately described without constant reference to its physiological correlates. In the present case the concept of drive arose in the description of behavior and is commonly used there when no knowledge of physiological conditions is available or thought necessary. The term attempts to deal with a special aspect of behavior, which, as we have seen, may be clearly defined, and with which we are here exclusively concerned. Doubtless the experimental result that we have obtained has a physiological significance, but its bearing upon the present discussion is negligible.

While it is obvious that the word drive refers to a *thing,* this is, from the present point of view, a verbal accident. So far as our observations are concerned, it is enough to say that an animal is hungry; it adds nothing to say that it possesses a certain amount of hunger. The use of the adjective is, of course, warranted by our present interpretation. The use of the substantive may be justified as a convenient hypostatization, although its use is likely to be abused. *Drive* never presents itself for investigation nor for measurement; the immediate experimental material is *behavior.* To assume that some measured aspect of behavior is a measure of drive is to carry the concept beyond any experimental justification, since, before a common factor of this sort can be set up to account for a group of observed aspects of behavior, it must be shown that all the variations

for which a change in the factor is supposedly responsible are of the same nature. Since this can be accomplished only through the investigation of the order of variation of particular instances, where the independent variable is a condition of an experiment; and since this is, as we have seen, the form that our investigation must take anyway, the concept of drive seems to have no experimental usefulness. The same criticism may, of course, be applied to the concept of reflex strength, if that term is taken beyond the simple operational definition that we have given it.

Summary

I

When we use the term hunger we are describing the presence or absence, the display or want of display, of certain characteristic behavior. The central problem in the study of hunger is to account for the appearance of either case at a given time. The mere observation of whether or not a rat eats is not, however, an adequate experimental measure, since it neglects the matter of degree of hunger. An adequate measure of those characteristics of behavior upon which we base a judgment of degree is available in the rate at which a rat eats.

II

Under the experimental conditions described herein, the rate at which a white rat eats a daily ration of a standard food varies in a uniform way. The amount of food (N) eaten at any time (t) during an eating period is given by the equation

$$N=Kt^n$$

where K and n are constants.

A compensatory increase in rate follows any delay which tends to bring N below its predicted value.

III

The question of hunger arises from an apparent variability in the behavior of the intact organism. The significance of a problem of this sort has been discussed elsewhere in connection with the concept of the reflex. In reflex terminology, the problem of hunger presents itself as a variation in the strength of certain reflexes concerned with the ingestion of food. A solution is suggested, using the present experimental material as an example.

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L'IMPULSION ET LA FORCE DU RÉFLEXE

(Résumé)

I

Quand on emploie le terme faim, on décrit la présence ou l'absence, la manifestation ou le manque de manifestation, d'un certain comportement caractéristique. Le problème central dans l'étude de la faim c'est d'expliquer l'apparence de l'un ou l'autre cas à un temps donné. La seule observation si un rat mange ou s'il ne mange pas, n'est pas cependant une mesure expérimentale adéquate, puisqu'elle néglige le facteur degré de faim. Une mesure adéquate de ces traits de comportement sur lesquels on base un jugement du degré est à trouver dans la vitesse à laquelle un rat mange.

II

Dans les conditions expérimentales ici décrites, la vitesse à laquelle un rat blanc mange une portion quotidienne d'un aliment ordinaire varie d'une manière uniforme. La quantité de nourriture (N) mangée à n'importe quelle heure (t) pendant une période de manger est donnée par l'équation

$$N = K\, t^{n}$$

où K et n sont constants.

Une croissance compensatoire de la vitesse suit n'importe quel délai qui tend à réduire N au-dessous de sa valeur prédite.

III

La question de la faim remonte à une variabilité apparente dans le comportement de l'organisme intacte. La signifiance d'un problème de cette sorte a été discutée ailleurs à l'égard du concepte du réflexe. Dans la terminologie du réflexe, le problème de la faim se présente comme une variation dans la force de certains réflexes qui entrent dans l'ingestion de la nourriture. On suggère une résolution, employant cette matière expérimentale comme exemple.

Skinner

TRIEB UND REFLEXENSTÄRKE (REFLEX STRENGTH)

(Referat)

I

Gebraucht man das Wort 'Hunger,' so bezeichnet man damit die Gegenwart oder die Abwesenheit die Offenbarung oder den Mangel an Offenbarung einer gewissen charakteristischen Tätigkeit. Die Kernfrage bei der Untersuchung des Hungers ist es, das Erscheinen der einen oder der anderen Situation zu einer besonderen Zeit zu begründen. Die einfache Beobachtung, ob eine Ratte frisst oder nicht, stellt jedoch kein genügendes experimentelles Verfahren dar, da sie den Grad der Stärke des Hungers vernachlässigt. Es steht uns aber in der Schnelligkeit, mit der die Ratte frisst eine genügender Massstab jener Kennzeichen der Tätigkeit zur Verfügung worauf wir eine Abschäzung begruenden.

II

Unter den hier beschriebenen experimentellen Bedingungen variirt die Schnelligkeit, mit der eine weisse Ratte eine tägliche Portion einer Normalspeise frisst, auf eine uniforme Weise. Es lässt sich die Quantität der zu einer gewissen Zeit (T) wärend der Fressperiode eingenommenen Nahrung (N) durch die Gleichung

$$N = Kt^n$$

darstellen, worin K und n Konstante sind.

Auf irgend eine Verzögerung die N unter ihren hervorgesagten Wert zu ziehen neigt folgt eine kompensatorische Beschlunigung der Schnelligkeit.

III

Die Frage des Hungers hat ihren Ursprung in der scheinlichen Variabilität des unversehrten Organismus. Die Bedeutung einer solchen Aufgabe ist anderswo besprochen worden, im Zusammenhang mit dem Begriff des Reflexes. In der Reflexenterminologie stellt sich die Aufgabe des Hungers dar als eine Variation in der Stärke gewisser Reflexe, die mit der Einführung von Nahrung zusammenhängen. Es wird eine Lösung vorgeschlagen, wobei das gegenwärtige Material als Beispiel dient.

SKINNER

DRIVE AND REFLEX STRENGTH: II*

From the Psychological Laboratories and the Laboratory of General Physiology of Harvard University

B. F. SKINNER[1]

It has been shown that the rate at which an albino rat of a particular strain eats uniform pieces of a standard food varies as a function of time from the beginning of an eating period, in such a way that the amount of food (N) already eaten at any time (t), measured from the beginning of a period, is given by the equation

$$N = Kt^n$$

where K and n are constants and n has a value lying between 0.68 and 0.70 (1,3). A rate of eating,[2] expressed as amount of food consumed per unit time, has only an indirect reference to the detailed behavior of the organism. A given rate is necessarily determined, however, by specific factors in behavior—of which the rate is, in consequence, a measure. Certain of these factors have been discussed elsewhere (1), and it has been found convenient to describe them and their changes in terms of the *strength* of the reflexes that are operative when a rat approaches, seizes, chews, and swallows a bit of food.

The stimuli for the initial members of this sequence of reflexes emanate from the food or the food tray. They act continuously. But the initial reflexes are not continuously elicited. In accordance with the principle of chaining, a first response will have supplied stimuli for the elicitation of succeeding responses, which carry the food to the mouth, chew and swallow it; and some at least of these later members of the chain utilize the effectors necessary for the initial responses and are prepotent over them. When the effectors are again free, the initial stimulus may take its effect.

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[1]National Research Council Fellow.

[2]Since the eating progresses by discrete steps we are justified in dealing only with an average rate over a finite interval of time. It is often convenient, however, to obtain such a value by measuring the slope of a smoothed curve or by differentiating the equation given above.

Usually, however, a further interval may be observed before the initial reflexes are again evoked. During this interval the rat may stand quite still or may make incidental responses to parts of the apparatus. But it will not respond to the food or food tray, in spite of the fact that the stimulating energies arising from these sources are unchanged.[3] The duration of such a period, under the conditions of the present experiments, may range from a second or two to several minutes. The termination is well marked: the initial response is elicited very suddenly, and the other members of the chain then follow in turn.

Such a period of irresponsiveness is by definition a refractory phase.[4] And since, more specifically, it is the refractory phase of the initial reflex, its proper measure is the total interval between homologous points in the course of two successive elicitations. The actual eating behavior must be regarded as running concurrently with the first part of the phase and ordinarily, therefore, as playing no part in delaying the repetition of the initial response. Nevertheless, if for any reason the refractory phase is briefer than the eating time, the latter will determine the recorded interval. This may happen in two sorts of cases: (*a*) where the chain has been made unduly long, as, for example, by the addition of arbitrary members (see the case noted below), or (*b*) where the reflexes are very strong and the refractory phase consequently very brief. We need not consider these special cases here. In the experiments referred to (1,3) and in those to be described the intervals seem to be determined solely by the refractory phase. This is certainly true at the lower rates of eating, where the behavior is more easily inspected, and seems also to be true of the highest rates here dealt with, which

[3]It is not quite correct to say that the stimuli for the initial reflexes do not vary, for some such variation must result from the activity of the rat during the period of irresponsiveness. If the rat has been thoroughly conditioned to the apparatus, however, the effect of this activity is negligible, for the incidental "investigatory" responses have then in the main adapted out and every part of the apparatus will evoke responses carrying the rat to the food tray. In any event the only possible effect of such a variation in the intensity of the stimulus is to scatter the observed times for a group of comparable intervals, and this scatter is actually very small, as may be inferred from the accompanying records.

[4]The terms reflex, reflex strength, refractory phase, and so on are used here in accordance with the definitions discussed in (2). In the present case the distinction between a relative and an absolute refractory phase cannot be maintained, since a measure of the strength of the response or of the stimulus is lacking.

are by no means maximal. If the contrary were true—if at the higher rates the refractory phase were obscured by the eating time—the curve for a decline in rate should show a break where the phase emerges to control the interval. Nothing of this sort is observed, however. It is possible that the eating behavior may have some effect upon the refractory phase of the initial response (we have noted that common effectors are employed), but in such a case the measured interval is, nevertheless, still a refractory phase.

The presence of a refractory phase and its operation to control the rate of eating lends a special importance to the initial reflex and raises the question with which the present experiments are concerned. Is the law expressed in the equation $N=Kt^n$ independent of the particular reflex that initiates the eating behavior? The usefulness of the law will depend largely upon the answer; for while the later steps in the process of ingestion (chewing, swallowing, and so on) vary but little between comparable organisms, the early steps (which are *conditioned* reflexes) may show an infinite variety.

In the experiments previously reported, rats obtained food from a small tray. A lightly balanced door, necessary for recording the behavior, partly closed the opening to the tray, but after the usual adaptations had taken place, the presence of the door modified the behavior only slightly. In such an instance the act of eating a pellet of food began with certain reflexes of approach to the tray and food, which brought the lips of the rat into contact with the food and thus set up the subsequent reflexes of chewing and swallowing. The plan of the present experiment is to add an arbitrary initial member to this sequence. The food tray is accordingly replaced by a repeating "problem box," which delivers a pellet of food into an open trough each time a horizontal lever is pressed downward. The response to the lever is thus added to the sequence. In the present experiments the necessary conditioning has already been acquired. An account of the behavior during the conditioning period is reserved for a later communication.

II

The problem box is shown in Figures 1*A* and 1*B*. It is about 17 cm. high and is constructed of galvanized iron. The trough from which the rat eats is 9 cm. x 5 cm. at the top and 3.5 cm. deep. The sides slope inward and nearly meet at the bottom. The horizontal lever, which must be pushed about 1 cm. downward in ob-

taining food, is at rest 7 cm. from the floor. Its inside edge is free of the side of the box by about 6 mm. The shields at the corners of the box expose 8.5 cm. of the lever at the front and about 4 cm. at the sides. The sides may be ignored, for only the front section is used by the rat. The lever is held in its resting position by a spring, which is inside the box and is not shown in the drawings. The tension of the spring is adjusted until a weight of 10 gms. will just force the lever downward to the position at which a contact is made. The lever will move somewhat past this point. The mercury-cup contact is shown at *A* in Figure 1*B*. A food magazine, bolted to the back of the box, is also shown in Figure 1*B*. A piece of food discharged from it rolls forward into the trough, where it is accessible to the rat.

The construction of the magazine is shown schematically in Figures 1*B* and 1*C*. The magazine proper is an upright glass tube 90 cm. long. It holds about 130 pieces of a standard food (McCollum's formula). The food is prepared with a modified "pill-machine," which produces cylindrical pellets 7 mm. long and 3 mm. in diameter. They slide freely in the tube and are released from it one at a time by an electrically operated escapement, the plan of which is also given in the figures. In constructing the escapement two small holes about 8 mm. apart are ground into the tube at its lower end. Through these the ends of a small semicircular piece of spring wire (*B*) are free to pass. The middle of the wire is fastened to a soft-iron armature (*C*) which swings about an imaginary line passing through the center of the tube perpendicular to its long axis. The line also passes through the center of the semicircle of the wire. When the armature is in its resting position, as shown in Figure 1*B*, the lower end of the wire is in the tube. The column of food rests upon it. When the armature is drawn up by the magnet (*D*), the lower end is withdrawn from the tube and the upper inserted to press against the second piece of food. The first piece is released.

The method of recording has been described (1). A writing point, tracing upon a slow kymograph drum, is lifted vertically a single uniform step at each contact made at the problem box. The graph of a series of contacts is a step-like line, the slope of which (that is, the tangent of the angle with the horizontal) is a measure of the rate. The rates shown in the accompanying graphs vary by a factor

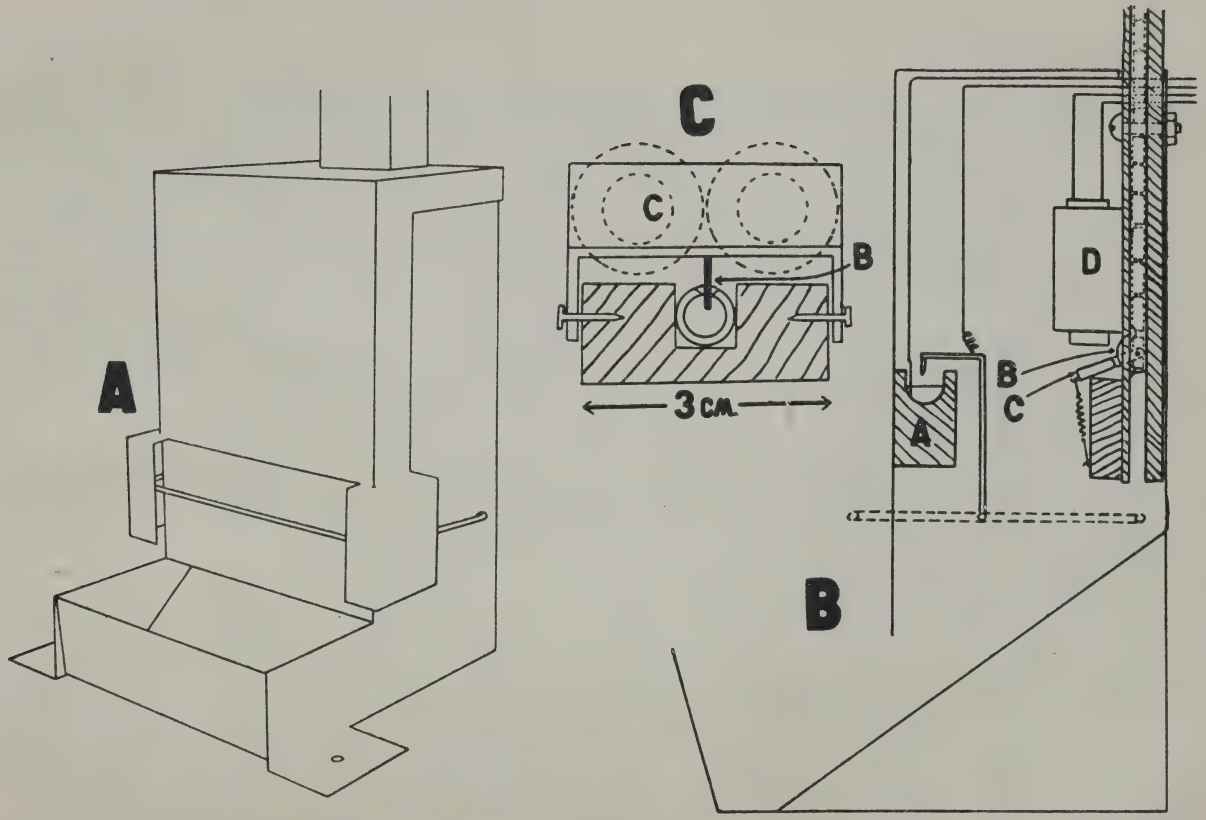

FIGURE 1

A. Sketch of the problem box.
B. Schematic section of the problem box, showing also the mercury-cup contact and the food magazine.
C. Cross-section of the magazine and escapement. For details see text.

of about four. In order to avoid superfluous contacts due to the inexpertness of the rat's manipulation of the lever, a device also previously described (1) is introduced into the circuit. Its effect is to break the circuit for a few seconds following each contact. In the wiring of the whole apparatus the solenoids of the magazine, recording device, and circuit-breaker are in parallel; the contacts in the problem box and circuit-breaker lie in series between the solenoids and a storage battery. To summarize the action of the apparatus:

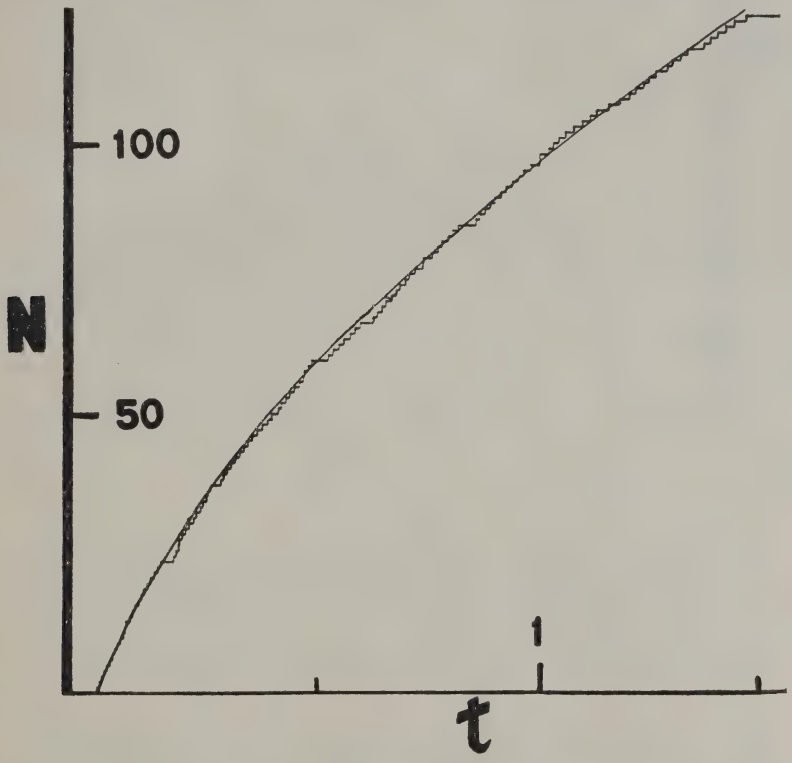

FIGURE 2

Kymograph record of the eating behavior of a white rat, where the pellets of food are obtained by pressing the lever of a problem box. N = number of pieces of food eaten; t = time from the beginning of the eating period in hours. The step-like curve is reproduced directly from the record. Coördinates and a theoretical curve have been added. The theoretical curve is for the equation $N=Kt^n$, where $K=2.0$ (units arbitrary) and $n=0.70$.

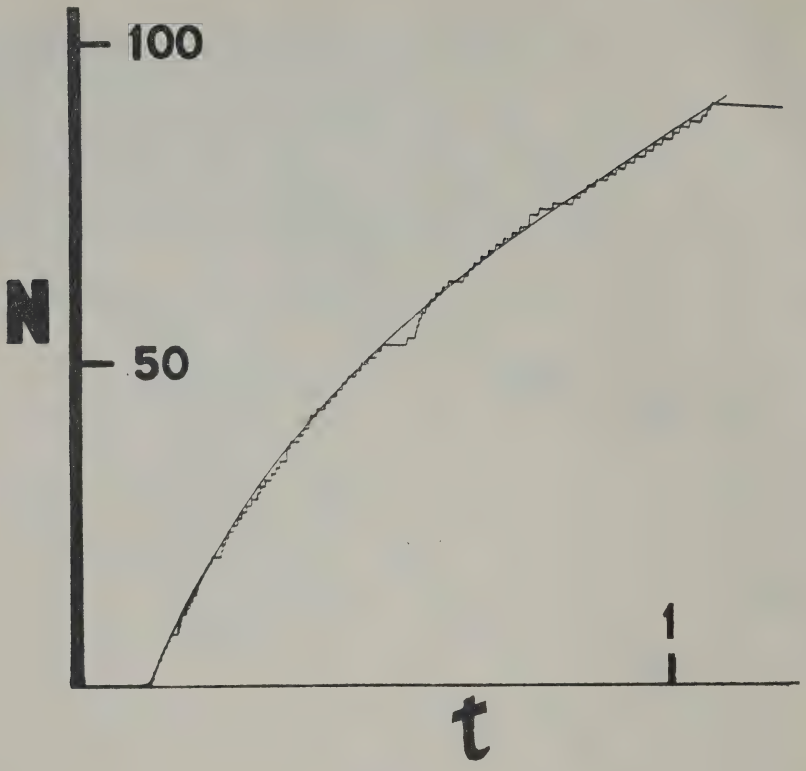

FIGURE 3

Kymograph record of the eating behavior of the same rat, where pellets of food are obtained from a tray. N = number of pieces of food eaten; t = time from the beginning of the period in hours. The curve is reproduced from the original record as in Fig. 2. The theoretical curve is for the same equation as in Fig. 2 and for the same value of n. In this figure K=1.9 (units as in Figure 2).

Note the delay and subsequent recovery midway along the curve.

when the rat presses the lever of the problem box, (*a*) a pellet of food is released into the trough, (*b*) the writing point is lifted one step, and (*c*) the whole circuit is thrown open for the following six or seven seconds.

The experimental procedure is in general as previously described (1). The problem box is placed in a sound-proof dark box 55 cm. x 50 cm. x 20 cm., which is continuously ventilated and contains a supply of water. At the same hour daily a rat is put into the box

and left there until a time at which no eating has taken place for about fifteen minutes. It is then removed, and except for about five grams of lettuce leaf no other food is given to it.

III

A typical record obtained with this method is given in Figure 2. The graph itself (the step-like line) and the t axis are reproduced directly from the kymograph record. The N axis and a theoretical curve have been added.[5] The record describes the behavior of the rat for a period of an hour and a half, during which 124 pellets of food, weighing in all about eight grams, are eaten. It will be seen that the rate begins at a maximum and declines steadily throughout the period. The experimental curve follows very closely the theoretical curve drawn through the data. The latter is for the equation $N=Kt^n$, where $n=0.70$ and $K=2.0$.

For comparison a curve obtained with the same rat, but where food is taken from a tray described in (1), is given in Figure 3. The theoretical curve is for the same equation and for the same value of n. In this figure $K=1.9$.

The value of K depends upon several conditions of the experiment and upon the animal employed (3). In the same situation and with the same animal it changes from day to day.[6] Consequently, the values here obtained (2.0 and 1.9) do not imply any essential difference between the curves. The constant n, on the other hand, has been found to be practically independent of the conditions of an experiment and of the animals employed (three generations of an inbred strain have now been tested). It is significant, therefore, that the same value of n is obtained in each of the present records. The two curves, that is to say, are practically identical.

A few special aspects of the records may be noted. The last horizontal line in each case might have been extended considerably to the right, for with the last recorded pellet the rat ceased eating

[5]When a record is taken, the vertical line described by the writing point is always ascertained to be at an angle of 90° with the horizontal. Because of a reconstruction of the recording apparatus the units of the present record differ from those of the records previously published.

[6]In calculating the values of K and n a record is transferred to coördinate paper and the values of N and t are read directly in the units of the paper. The constant n is independent of this procedure. The value of K, however, depends upon the units chosen, and two values can be compared only if the procedure has been the same. The present magnitudes of K are not to be compared directly with those previously published.

altogether. The curves, therefore, end abruptly. This is typical of records obtained with either procedure. An example of another characteristic, which has already been discussed (1), is offered by Figure 3. At about the fiftieth piece of food the rat stops eating for a short period and consequently falls slightly behind the schedule set by the equation. The delay is followed, however, by a sharp acceleration, which brings the curve back to its proper position. After the first step, which is exceptional, the recovery curve is convex upward and remarkably uniform in its curvature. Note that its initial rate is greater than that of the beginning of the main record.

IV

In the two cases we have compared, then, the validity of the equation $N=Kt^n$ is independent of the nature of the initial reflex. And this seems to be true in spite of the fact that it is the elicitation of this reflex at the end of its refractory phase that determines the rate of eating. The two present responses, moreover, are quite unlike, and the broader conclusion seems warranted that the rate of change of the rate is in general independent of whatever reflex may initiate the behavior.

An obvious exception to this rule will arise if the added member consumes so much time that the maintenance of a high rate is impossible. A case of this sort has appeared in some unfinished experiments by M. F. Stevens and the writer. Here a rat is conditioned to run ten turns in a running wheel (a distance of approximately 60 feet), at the end of which a pellet of food is automatically delivered to a tray at the side of the wheel. The running must be repeated for each pellet. It is obvious that the higher rates at the beginning of the normal curve cannot be reached under these conditions. One obtains, therefore, a limiting slope at the beginning of the record. A more detailed account may be reserved until the experiments are completed.

The present result justifies a simplifying assumption made both in this and in the preceding paper (1). We have dealt with a rate of eating in terms of the characteristics of an initial reflex. As a matter of fact, there is no one initial reflex of this sort. We cannot control the behavior of the rat adequately enough to insure the administration of an invariable stimulus or the elicitation of an invariable response. The number of possible initial reflexes, even in the simple act of picking up a pellet of food, is therefore indefinite, for it corresponds to the number of possible ways in which the stimu-

lating energies emanating from the food may affect the organism. We are justified in speaking of *one* initial reflex only because the members of this indefinite group possess very much in common. In particular, they all affect the recording apparatus in the same way, and all lead to the same process of ingestion of the pellet. Why this diversity does not affect the regularity of the recorded behavior is made clear by the present result. The change in rate is independent of the precise nature of the initial reflex.

Summary

At the normal rates of eating dealt with in certain experiments previously reported, a given rate is determined by the refractory phase of the reflex initiating the behavior. It is important to know whether the change in rate implied in the law $N=Kt^n$ is dependent upon the nature of this initial reflex.

The previous experimental procedure is modified, therefore, so that the rat obtains uniform pieces of food in operating a repeating problem box. The initial response to the lever of the problem box is thus added to the usual behavior.

The rate at which the lever is manipulated during an eating period is measured in the usual way. Typical curves obtained in both procedures are reproduced for comparison. Both are fitted with theoretical curves, which are for the same equation and the same value of n. The curves are thus essentially identical.

Conclusion. For the two reflexes here compared, and probably in general, the rate of change of the rate of eating is independent of the nature of the particular reflex with which the eating behavior begins.

I wish to express my indebtedness to the Department of Psychology, Harvard University, especially to Dr. Morgan Upton, for the space and facilities necessary for these experiments.

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L'IMPULSION ET LA FORCE DU RÉFLEXE: II

(Résumé)

Aux vitesses normales de manger étudiées en certaines expériences déjà rapportées une vitesse donnée est déterminée par la phase réfractaire du réflexe qui initie le comportement. Il est important de savoir si le changement de vitesse impliqué dans la loi $N = K\,t^{n}$ dépend de la nature de ce réflexe initial.

Le procédé expérimental antérieur est donc modifié, de sorte que le rat obtient des morceau uniformes de nourriture dans son opération d'une boîte à problèmes qui se répètent. La réponse initiale au levier de la boîte à problèmes est ainsi ajoutée au comportement.

On mesure de la manière ordinaire la vitesse à laquelle le levier est manipulé pendant une période de manger. On reproduit pour la comparaison les courbes typiques obtenues dans les deux procédés. Les deux sont pourvus de courbes théoriques, qui sont pour la même équation et la même valeur de *n*. Les courbes sont ainsi essentiellement identiques.

Conclusion. Pour les deux réflexes ici comparés, et probablement en général, la vitesse du changement de la vitesse de manger ne dépend pas de la nature du réflexe avec lequel commence le comportement de manger.

SKINNER

TRIEB UND REFLEXENSTÄRKE, II

(Referat)

Bei normalen, in früher besprochenen Experimenten behandelten Fressschnelligkeiten (rates of eating) wird eine gewisse Schnelligkeit durch die refraktäre Phase des die Tätigkeit anregenden Reflexes bestimmt. Es ist wichtig festzustellen, ob die in dem Gesetz $N = Kt^{n}$ mit einbegriffene Aenderung der Schnelligkeit von der Beschaffenheit dieses anregenden Reflexes abhängig ist.

Das vorherige experimentelle Verfahren wird also so modifiziert, dass die Ratte gleichförmige Stücke Nahrung bei der Handhabung eines widerkehrenden Aufgabekastens (repeating problem box) erhält. Die Anfangsreaktion (initial response) auf den Hebel des Aufgabekastens wird also der Tätigkeit zugefügt.

Die Schnelligkeit, mit der der Hebel wärend einer Fressperiode gehandhabt wird wird auf die gewöhnliche Weise gemessen. Es werden typische, mit beiden Verfahren erhaltene Kurven zur Vergleichung wiedergegeben. Beiden werden theoretische Kurven angepasst, die der selben Gleichung und dem selben n-Wert angehören. Die Kurven sind also wesentlich die gleichen.

Schluss: Bei den zwei hier verglichenen Reflexen, und wahrscheinlich im Allgemeinen, ist die Schnelligkeit der Veränderung der Fressschnelligkeit von der Beschaffenheit des bestimmten Reflexes, mit der die Fresstätigkeit anfängt unabhängig.

SKINNER

THE SENSORY ASPECTS OF ELECTRICAL STIMULATION*

From the Psychological Laboratories of Clark University

MASON N. CROOK

INTRODUCTION

Electricity is not the normal stimulus for any sense-receptor, and perhaps for this reason has not been widely used in the study of sensory experience. We have a growing psychogalvanic literature, and in physiology electrical stimuli have been widely used as a means of excitation, particularly of motor nerves. From such sources we are gradually gaining knowledge of the electrical properties of living tissue, but the study of the experience of electrical stimulation as such has, on the whole, been neglected. Such limited experiments as have been conducted will be referred to more specifically in the course of this paper.

In electricity we have a form of energy which is convenient, controllable, and capable of stimulating the receptors, or at least the afferent nerves, of any sense modality, which should prove useful in the investigation of the salient problems of sensory reception.

The present research was undertaken, not as a direct attack upon these salient problems, but as a preliminary investigation of certain phases of the experience of electrical stimulation of the skin, in an effort to obtain an evaluation and orientation of it relative to other sensory experiences. Such an evaluation and orientation should put us in a better position to use electrical stimulation in the solution of the problems mentioned above.

APPARATUS

The source of the current used in all the observations here described was the 110-volt, 60-cycle, alternating current from the municipal power plant.

Two different arrangements were tried. In the first, a small transformer was used, to which the main wires were led and from

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which different voltages up to 25 could be taken. The secondary of the transformer was put in circuit with a switch, a slide rheostat with a resistance of 3100 ohms, a milliammeter, and two chinaware cups of salt water. The gap across the cups was closed only when the observer had his fingers in the salt water. The observer sat at the table with the two cups before him. Into each cup dipped an end of wire to which a copper penny was soldered. Behind the cups was a screen which shielded the rest of the apparatus from the observer. The experimenter sat at the other side of the screen, with the transformer, the switch, the milliammeter, and a stop-watch before him. The experimenter controlled the intensity of the current by arranging the setting on the transformer and by sliding the contact point on the rheostat. In this way practically any voltage desired, up to 25, could be obtained. The problem was not concerned with the resistance of the observer, so in the regular series only the amperage was recorded. A multi-range Paul milliammeter was used. This instrument has four ranges varying from 2 to 100 milliamperes. In practice, all the currents used were found to be of the order of 1 milliampere, so the lowest scale was used for reading in most cases.

With the set-up just described it was very difficult to keep the current constant for any length of time. In a period of ten minutes of continued stimulation with a current of .8 milliampere, a rise of .03 or .04 milliampere might occur. Moreover, it was practically impossible to obtain desired intensities without testing the current and adjusting the instruments every time. Such changes in the current were presumably due to changes in the resistance of the skin, rather than to changes in the potential of the source itself. The voltage of the main current was read at intervals over a period of 24 hours and was found to have a diurnal variation of about 5%, with a wholly insignificant variation within the period of any one observation. However, it is known that skin resistance varies within wide limits, and it is known that it decreases with continued electrical stimulation (4). Therefore the apparatus was modified in the following way to reduce this effect of varying resistance to a minimum, and this modified arrangement was used in most of the observations reported.

The essential feature of the modification was the elimination of the transformer and the use of the 110-volt current directly, enough resistance being put into the circuit to reduce the amperage to the

desired amount. This required a variable resistance (in the neighborhood of 100,000 ohms) which was provided by water rheostats. Each water rheostat consisted of a glass U-tube filled with tap water containing a drop of sulphuric acid. Each side of the U-tube carried a stopper through the center of which ran a brass rod that could be raised or lowered in the water at will. The depth of the rods in the water determined the amount of resistance. When the double circuit described below was arranged, four U-tubes in all were used. It was found that, in spite of the high resistance per inch of column of water, the current could be adjusted by this means as accurately as it could be read on the milliammeter.

The essential advantage of the new arrangement was that, in a circuit having a total resistance of over 100,000 ohms, the changes in resistance which occurred under the experimental conditions were relatively insignificant and caused no marked change in the current. The resistance through the fingers under the conditions of observation must have been, as estimated on the basis of a preliminary measurement made under slightly different conditions, about 15,000 ohms. Taking this as approximately correct, it constituted nearly all the resistance in the circuit as the apparatus was first arranged, and a change in it of 10% would cause a corresponding change of 10% in the amount of current. Under the new arrangement, the finger resistance constituted not more than one-fifth, and usually less, of the total resistance in the circuit, and a 10% change in it was not more than a 2% change in the total resistance. Consequently, the current remained relatively constant.

The essential elements of the apparatus as described above remained the same throughout. However, certain of the observations involved the presentation of two stimuli of different intensities to the same hand in immediate succession. For this purpose a compound switch was so arranged that it threw out one resistance and threw in another without changing the circuit in other respects.

In most of the observations stimulation of the index finger alone was obtained by having a small contact surface on the index finger itself and a large contact surface elsewhere on the hand. Under these conditions the current was felt only in the index finger.

The method finally adopted of getting a uniform contact surface for the finger was as follows: The finger projected down through a hole in a board over the top of the cup. The board was soaked with

paraffine, so that it would not absorb water, and the salt solution in the cup was kept always at the same level. The hole was of such a size that the finger slipped down easily to the second knuckle, and the solution came up to about the middle of the second joint. In all of the series, except those with N, the index finger projected through the hole in the board into one cup, as described above, and the three other fingers of the same hand were immersed in the solution in the other cup.

Observers

The observers used in the experimental work to be described were Dr. J. P. Nafe (N), Professor of Psychology at Clark University, Dr. D. E. Johannsen (J), instructor in Psychology at Wellesley College, W. Dennis (D), Assistant Professor of Psychology at the University of Virginia, N. Goldman (G), and R. Leeper (L), graduate students in Psychology at Clark University, and the writer, (C), graduate student at Clark University.

Analysis of the Experience

For the purpose of obtaining a qualitative description of the experience of electrical stimulation four observers were used, G, J, L, and N. G and L had never before observed on electrical stimulation. J and N had both received some experience in preliminary observations on the present problem, and had both previously observed in experiments on other forms of skin experience. Each of the four observers was presented, at a single sitting, with a series of progressively increasing intensities irregularly spaced relative to each other. Observer J was presented with each intensity at least twice. A short recovery period was allowed after each stimulation. Except with observer N, most of the stimuli were presented for 30 seconds. Very weak stimuli adapted out in less than 30 seconds; a few of the stronger ones were presented for longer intervals. With N the transformer set-up was used, and the two index fingers were stimulated together by having one immersed in each cup. With all the other observers the high resistance set-up was used, only one index finger being stimulated. Instructions were as follows:

> "You will be presented with a small electrical stimulus through the contact at the end of your index finger. You are to describe the experience as completely and accurately as you can."

No suggestions were given the observers as to choice of terminology. Painfully strong intensities were not used extensively because it was not desired to cloud the sensory experience with an affective tone.

Descriptions of the experience by the observers are quoted verbatim, except for slight editing in the interests of intelligibility.

Reports by the four observers on the first three intensities used in each case are given below. Observer J was presented with the first intensity four times and with the second intensity three times; in this case the first report obtained on each intensity is quoted.

Observer G. Stimulus 0.38 milliampere. Complete adaptation in 6 seconds.

> "I felt a vibration starting at the tip of the finger and moving up towards the knuckle, finally becoming localized in the knuckle—not continuous—discrete points all the way through —little jabs like holding the finger on a vibrating tuning fork, but each of the points is sharp."

Stimulus 0.46 milliampere. Complete adaptation in 9.6 seconds.

> "The time it took to go from the tip of the finger to the knuckle was very short—it seemed to jump."

Stimulus 0.55 milliampere. Duration of observation 30 seconds.

> "Practically the same, but it went higher—was localized on palmar side of third knuckle and seemed to stay there definitely. Still strong when it was shut off."

Observer L. Stimulus .27 ampere. Duration of observation 30 seconds.

> "It is most like two things—one is the feeling that your arm has gone to sleep, and the other is the feeling you get when your blood pressure is being taken—with the band around your arm—it gives you that dull, throbbing experience—sort of in waves—pulsations just about the rate of the pulse beat."

Stimulus .32 milliampere. Duration of observation 30 seconds.

> "Not so dull or diffuse that time—it was more localized on the ball of the finger—the feeling was continuous and wavered —about the shape that a flame playing up the side of your finger would cover, and of about that degree of variability. The similarity of this to the points of bright pressures described by Dr. Nafe is suggested, but I have the feeling that it is simply an analogy, and not very basic to it."

Stimulus .41 milliampere. Duration of observation 30 seconds.

> "Much more of the surface of the finger was covered—perhaps almost all of it that is in the water, with the exception of the back of the first knuckle. The feeling is more or less discrete—gave the impression of travelling inward instead of travelling up the finger as in a former case,—seemed to start at the surface and work in. The rate of pulsation was much increased. Another thing is that the temperature of the water and the cups seemed to suddenly decrease—that occurred to me 5 or 10 seconds after the stimulation started. There was a burst of stimulation at the start which didn't continue—seemed to decrease to 2/3 of its first intensity in 3 or 4 seconds. When it was turned off it disappeared sharply and clearly, in contrast to some of the earlier ones."

Observer N. Stimulus .1 milliampere.

> "Very mild—like very small bubbles hitting on finger—discrete things. Grows less noticeable but something is still present."

Stimulus .2 milliampere.

> "Localized definitely—more intensive—very much like the other—bubbles finer, but coming with more force—pressure pattern inside."

Stimulus .45 milliampere.

> "Distinct—pressure pattern inside finger, above second joint. Grainy, discrete, continuous, streaming, no direction, less intense in right."

Observer J. Stimulus .22 milliampere. Duration of observation 30 seconds.

> "When you first turn on the current the only sensation I get is a pressure of a rather pricky quality around the centre of the second joint. It is unintense, faint, fairly diffuse—I could not delimit it on either side—it appears as a band of pricky pressure."

Stimulus .37 milliampere. Duration of observation 30 seconds.

> "It is essentially the same thing as the other, but extends over a much greater area. The pressure seems to include almost the whole end of the finger—both joints. The moving pricks were felt not only in a band at the middle of the second joint, but also in a fairly lively pattern on the ball of the finger. It feels the way the Brownian movement looks."

Stimulus .63 milliampere. Duration of observation 30 seconds.

> "I can't think of any essential difference between this and the other. The pressure was more intense and covered the whole finger in the water. I think the pricks cover the same area, but there may be a place on the back where there are no pricks. They are very lively—the individual pricks are deeper."

Upon the basis of such evidence it can be claimed that electrical stimulation gives an experience of characteristic pattern, but not otherwise differentiated from other cutaneous experiences. All of the *O*'s speak of the discreteness and movement of the experience.

Values of stimuli used ran up to 1.4 milliamperes with N, 1.6 milliamperes with G and J, and 4.5 milliamperes with L. The main differences reported as the strength of stimulus increased are those of intensity and extensity. L and J report the highest intensities as somewhat painful, but J insists that there is no qualitative difference between these and the non-painful intensities. See the reports below.

Observer L. Stimulus 3.0 milliamperes. Duration of observation 30 seconds.

> "Felt it with the other three fingers—a rather dull ache in them. A reflex tendency to withdraw, as there has been the two times before, I guess. Might be called strong enough to be unpleasant—not so much so when it was going as in the first 5 or 10 seconds—very similar to previous one."

Stimulus 3.5 milliamperes. Duration of observation 30 seconds.

> "Shooting pulsations in the other fingers, particularly the nearest one—waves seemed to have constricting, tightening effect, not only lengthwise but also around the finger, particularly the third joint of the index finger. More intense, but no reflex jerk at the start."

Stimulus 4.5 milliamperes. Duration of observation 30 seconds.

> "All three extra fingers were equally involved—could feel current crossing from knuckle of the index finger to the others through the hand. More intense ache—not steady but throbbing, in the last half inch and near the third joint of the index finger. Not the first time that ache has appeared in that finger, but the first time it has become as definitely unpleasant."

Observer J. Stimulus 1.13 milliamperes. Duration of observation 30 seconds.

"I have the impression that the pressure part is the part that changes rather than the pricks. The last two or three times the pricks have been perhaps a little livelier and deeper, but the pressure has been decidedly more intense. It was slightly painful around the base of the nail. I think that the pain is dependent entirely on an increase in pressure, and not on the prick pattern (movement pattern)."

Stimulus 1.58 milliamperes. Duration of observation 5 minutes 8 seconds.

"No fundamental difference between this and the other. Pain around the nail is greater. No difference in pricks. Area is greater—extends over into three other fingers. Only distinction between pain and rest of pressure is of greater intensity (2 min. 35 sec.). Think pain is gone now—but hard to tell when it is gone—that is one indication that it is not qualitatively different. I think, as adaptation goes on, oscillatory movements tend to become slightly more rapid with a smaller amplitude."

Stimulus 1.59 milliamperes. Duration of observation 3 minutes 10 seconds.

"Oscillatory things grow up as if the amplitude increased, and then decreased with adaptation. I am sure there is no qualitative difference between the pressure and the pain. (2 min. 30 sec.). Pain is practically gone, but not quite. The pain here is more diffuse than a simple prick—covers a bigger area —strikes in deeper. I think of it as all through the bone—it is not just a surface thing. It is very different from having a tooth drilled—this experience is fairly well defined, but its edges are fuzzy. Like ache, although it is brighter than ache in some ways—perhaps because it is smaller and more condensed."

The two most experienced observers differentiate between the pattern of pricks and an element of dull pressure, though there is some uncertainty as to how independent the two phases are. See the report by J on 1.13 milliamperes quoted above, and the following report by N.

Observer N. Stimulus 1.4 milliamperes.

"Bubbles much as before—pressure increasing. Chiefly intensively and extensively greater—qualitatively the same. Little discrete things are all bright, pressure pattern isn't—little

> things not painful—movement with heart beat. Intensity reducing with time, but pressure phase seems worse—sand and gravel effect less and less discrete."

One explanatory hypothesis is that the pressure element is due to secondary stimulation from enhanced muscular tonus of the stimulated finger. At high intensities a feeling of release of pressure is sometimes felt with cessation of the current. However, the two phases of the experience may be due to direct stimulation of two classes of receptors. Martin and Porter (2) suggest that under certain conditions deep receptors are stimulated, and under other conditions, skin receptors. Localization by our *O*'s was sometimes, but not always, within the flesh. Ferrall and Dallenbach (1) found it possible to use electrical stimulation for the pain element in a synthesized experience of heat, which suggests that the effective stimulation under their conditions was stimulation of skin receptors. In the present problem an effort was made to throw light upon this problem by the method of partial anaesthetization of the skin.

Anaesthetization of the Skin

Anaesthetization was effected by cooling with ether until sensitivity was distinctly modified. Each of three *O*'s, J, L, and C, was asked to report upon three different intensities before and after cooling of the skin. The three intensities used were one just above the limen, one of medium strength, and one quite strong but not painful. The procedure was as follows: The stimulus was presented for one minute, then a recovery period of five or six minutes was allowed. During the recovery period the skin was cooled with ether until light jabbing with a needle no longer gave a sensation of prick. Then the same electrical stimulus was again presented. The *O* was instructed to report any perceptible change in the intensity or quality of the sensation. No very marked change was reported by any *O*, and no change at all in pattern or quality was reported. J and C reported that the lowest intensity seemed a little stronger after ether had been used; J reported that the medium intensity felt a little stronger after ether was used; C reported that the highest intensity seemed a little stronger, and L reported that it seemed a little weaker after ether was used. So out of nine comparisons, four show an apparent increase and one an apparent decrease in intensity of sensation after the skin had been partially anaesthetized. The conditions of observations were such that these results cannot be taken to indicate a definite

direction tendency. Since no marked change in the experience is found to accompany an impairment of the sensitivity of the skin, there is some ground for the inference that the receptors involved are largely subcutaneous. When a slight cut in the skin is exposed to electrical stimulation the *O* usually feels a sharp pain point localized at the cut. When a sufficiently strong current is presented through a small contact point on the normal skin a similar pain point is felt. It may be that moderate currents are felt only through the deep receptors, while strong currents stimulate also the skin receptors to give the experience of pain.

Of course, we must recognize the possibility that the effective stimulation is not in the specialized end-organs proper, but in the nerve-fibers serving these end-organs. Such a possibility does not invalidate the above discussion.

Adaptation

The process of adaptation is one of the most important phases of sensory experience. Therefore, we attempted to obtain some quantitative data on adaptation to the alternating current used as the stimulus in this experiment. An alternating current is, of course, an intermittent stimulus, giving rise to a new excitation presumably once with every alternation. It is well known that a sustained direct current stimulates only while it is undergoing a change. If the circuit is closed and the current left on at its maximum value, it will be felt only at the instant of making contact. Therefore, the change in the current is the effective stimulus. Continuous stimulation by a constant stimulus, as in vision, does not occur. However, if the alternating current is considered as a sustained series of discrete stimuli of constant magnitude, the adaptation of the organism to the series can be observed and measured. Under the conditions of this experiment, very weak stimuli adapt out completely. Stronger stimuli seem to reach a final level of adaptation beyond which there is no further change. There is reason to think that the same thing occurs in vision, and possibly also in some of the other sense departments. In the present experiment two different types of relationship were determined: (*a*) the relationship between intensity and time for complete adaptation of very weak stimuli, and (*b*) the relationship between time of adaptation and degree of adaptation for a constant stimulus.

The time and intensity relationship for stimuli weak enough to adapt completely was determined first for observer N. This was one of the earliest series of observations made in this study and consequently suffered from certain weaknesses of technique which were later eliminated.

The observer sat with each index finger in a cup of salt solution, and consequently both fingers were equally stimulated. Tips of the fingers rested lightly on the bottoms of the cups, boards to regulate the depth of the fingers in the solution not being used. Instructions were to report when the sensation first definitely disappeared in both fingers. E read the milliammeter at the beginning and end of each stimulation, and took the time with a stop-watch from the beginning of stimulation until *O* reported the disappearance of the sensation. Five minutes for recovery were allowed between observations. Enough observations had been carried out with slightly different methods to allow adequate training, so all the data obtained by this method are reported except those in which the current varied more than 0.02 milliampere during the observation. Results are presented graphically in Figure 1. The conspicuous variability is attributable to the conditions of observation described above. In spite of the variability, however, an unmistakable relationship can be observed, indicated approximately by the freehand curve which has been drawn in.[1]

This relationship was next determined on observer C. Only the index finger of the right hand was stimulated, the second contact being made through the three other fingers of the same hand. The paraffined board was used to keep the depth of the finger in the solution constant. After several observation periods, it was discovered that part of the variability in reports was due to the inadequacy of the recovery period of 5 minutes, so a variable recovery period was adopted graduated in the following way: when the immediately preceding stimulation lasted 0-5 minutes, the recovery period was 10 minutes; when the preceding stimulation lasted 5-10 minutes, the recovery period was 15 minutes; when the preceding stimulation lasted 10-15 minutes, the recovery period was 20 minutes; when the preceding stimulation lasted 15 minutes or over, the recovery period was 25 minutes. With this arrangement, no effect of incom-

[1]Data on which Figures 1, 2, and 3 are based appear in Table 1 of the appendix.

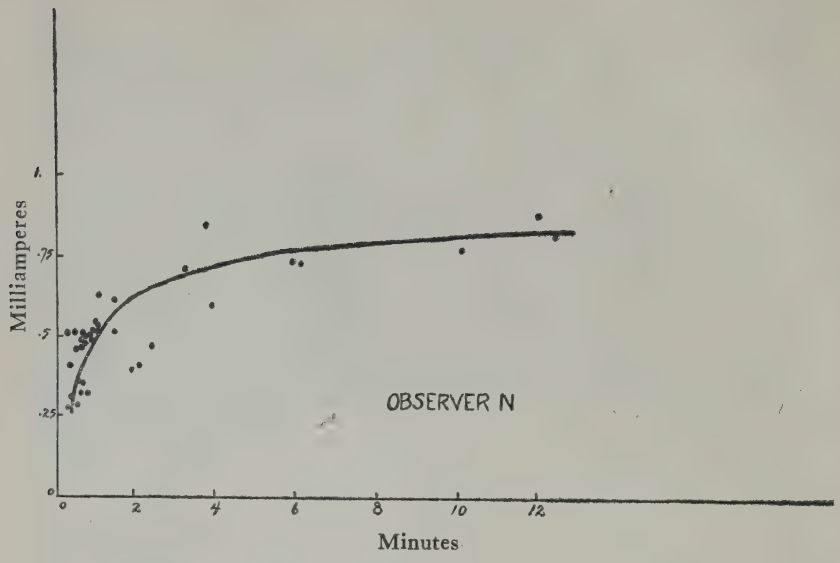

FIGURE 1

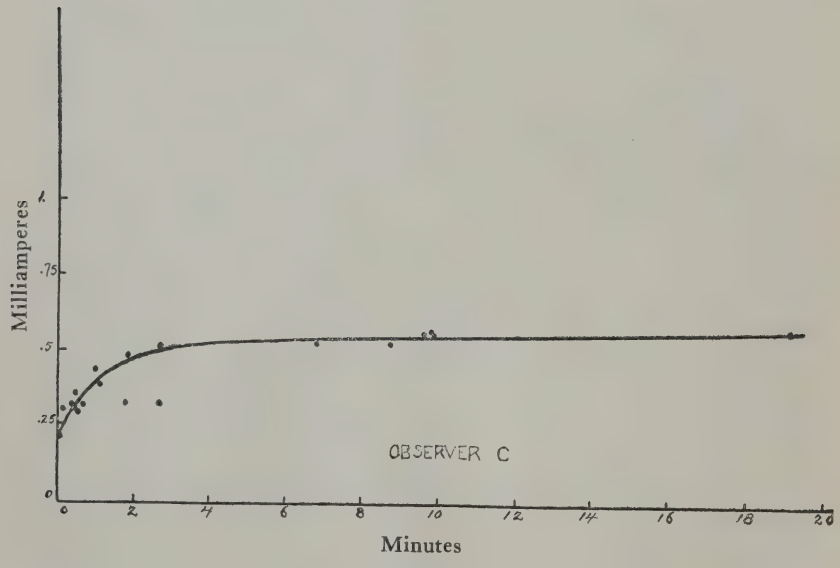

FIGURE 2

plete recovery could be observed by inspection of the data. Results from the first three observation periods were thrown out and all results obtained in the last three sittings under the above condition are reported, with the exception that observations during which the current varied more than 0.02 milliampere were thrown out as before. The relationship is presented graphically in Figure 2. All but two of the points fit the freehand curve very closely, and these two are doubtless exceptional cases which can legitimately be ignored.

The third observer used in this connection was D. The procedure was the same as with C. The first three sittings were considered as a training period and their data not used. Results from the last three sittings appear graphically in Figure 3. The freehand curve drawn through the dots appears to represent the relationship quite well.

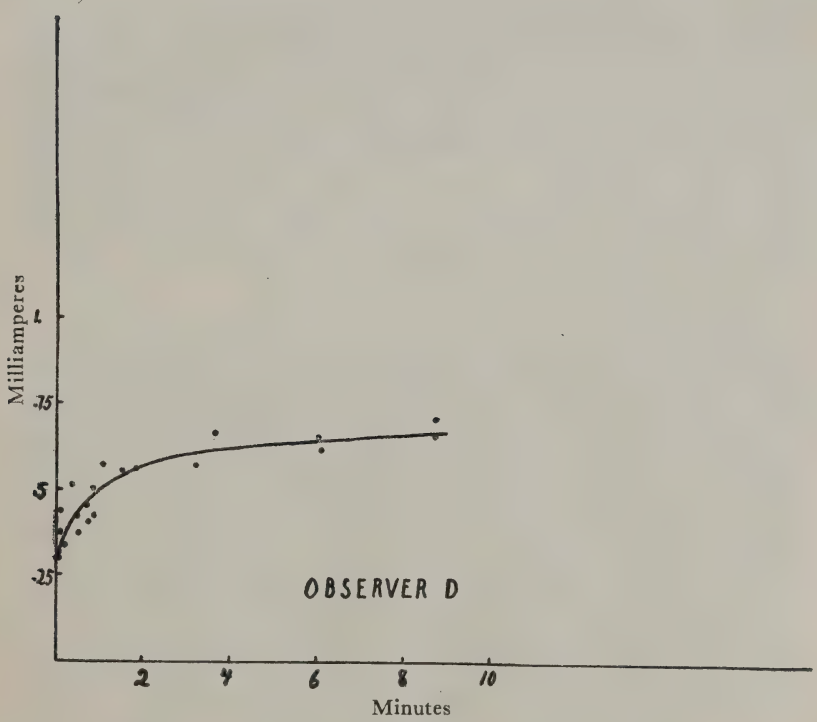

FIGURE 3

Upon inspection of these three figures, it appears that the curves approach asymptotes and the time for complete adaptation approaches infinity as the intensity increases. The asymptotic value varies between observers, but seems to be in every case below 1 milliampere.

An attempt to plot the adaptation curve itself was made with observer C. Because of difficulties of procedure, it was found impractical to measure the change in apparent intensity by the comparison method, so the change in the limen with sustained stimulation was measured instead. This is a different, but none the less real, measure of adaptation. As the sensitivity of the organism decreases with stimulation, the limen increases.

For this series of observations an adapting current of 1 milliampere was used. The procedure was as follows: *E* turned on the adapting stimulus for a given interval of time; at the end of that interval *E* threw the compound switch which broke the original circuit and completed another circuit, giving a stimulus of lower intensity to the same hand. The second current was left on for approximately 1/2 second. Thus *O* was presented with the adapting stimulus for a given period of time, then, after a dead interval of about one-tenth of a second, he was presented with a less intense stimulus in the same finger for 1/2 second. *O* reported upon the second experience as felt or not felt. Periods of adaptation used were 10 seconds, 30 seconds, and 60 seconds. Recovery periods after these three adaptation periods were 3, 4, and 5 minutes, respectively.

It was, of course, not necessary to make precise determinations of the limen by refined psychophysical methods, but only sufficiently accurate approximations to show the general trend of the change with time. The measuring of the limen before adaptation could be done quite precisely enough for our purpose with little trouble, for the positive and negative judgments overlapped by only about 0.05 milliampere. The judgments after adaptation, however, were much more variable, and it was found necessary to take a large number of readings at each point. The following procedure was finally adopted. A sufficient number of exploratory observations was made at each of the chosen intervals to get an approximate idea of where the true value lay. Then, for each adaptation interval, five intensities of stimulus were chosen covering such a range that the true value of the limen for that interval fell somewhere within them. Each of the three median intensities was presented ten times, and each of the

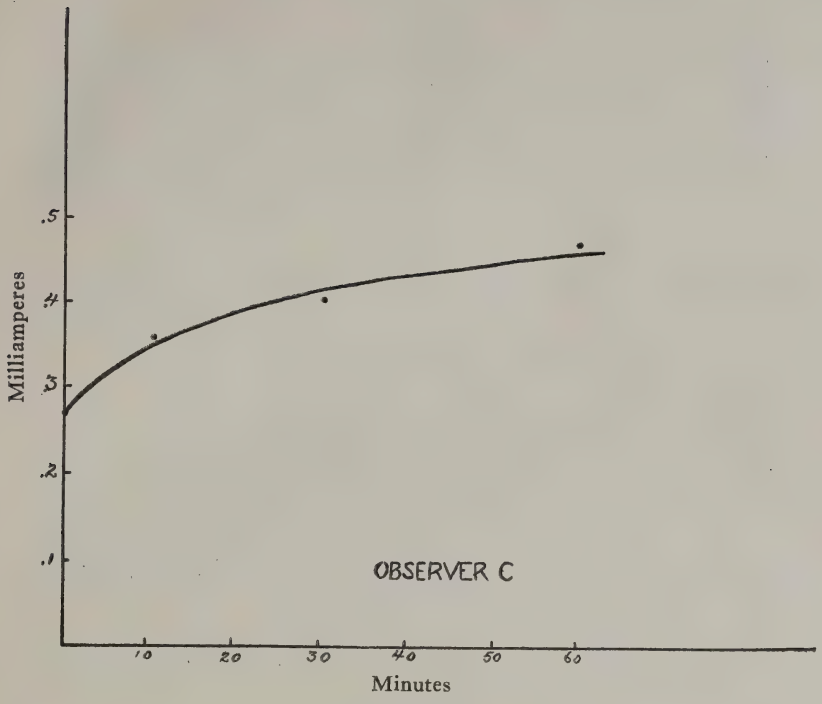

FIGURE 4

two extreme intensities was presented five times. For each intensity the percentage which the plus judgments made of the total number of judgments was computed. These five percentage figures were plotted on a chart scaled in milliamperes on the ordinate and in percentage of positive judgments on the abscissa. The points when plotted did not show a sigmoid relationship, so a straight line, which seemed to be the line of best fit, was drawn through them in each case. The point at which the straight line crossed the perpendicular erected at the 50% point on the judgment scale was taken to mark the true value of the limen on the milliampere scale. This is, of course, a rough approximation to the method of constant stimuli.

From the three values so determined and the value of the limen before adaptation the curve given in Figure 4 was drawn.[2]

[2]Data on which Figure 4 is based appear in Table 2 of the appendix.

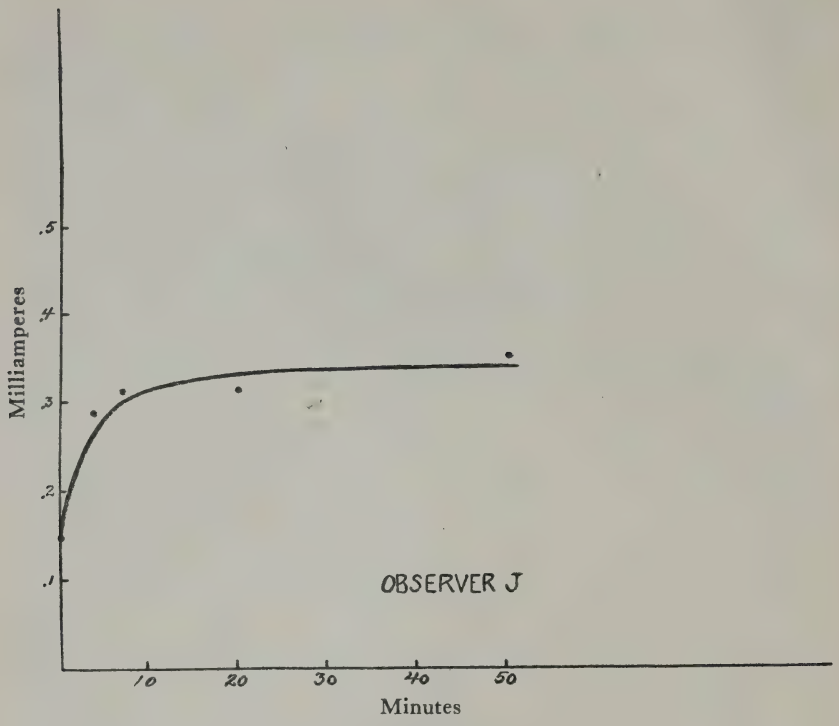

FIGURE 5

The adaptation process was also determined in the same way, with minor variations, for observer J. In this case an adapting stimulus of 0.5 milliampere was used. Four points on the curve besides the zero point were obtained—after 4 seconds, 7 seconds, 20 seconds, and 50 seconds of adaptation. Four intensities of constant stimuli were used at each adaptation interval, instead of five, and ten observations were taken at each intensity. Consistency of judgment was found to be much better than with C. Consistency was so good, and discrimination so precise, that percentages of plus judgments other than zero or 100 were obtained only for the two intensities nearest the true value of the limen at each adaptation interval. This made the drawing of the lines to obtain the 50% point perhaps a little less accurate than with C. The curve resulting from the points here obtained and the limen before adaptation is presented

in Figure 5.[3] It will be observed that the absolute limen is much lower than with C.

This part of the experimentation shows that the organism adapts in a very definite way to electrical stimulation. The fundamental nature of this process is not known. The curve seems to approach a final level in each case. A superficial similarity to adaptation in vision is apparent.

Summary

An investigation of the following sensory aspects of stimulation of the index finger of the right hand by means of a 60-cycle alternating current at about one milliampere was made:

1. the qualitative attributes of the experience.
2. the locus of excitation by electrical stimulation.
3. the relation between intensity of stimulus and time for complete adaptation for very weak stimuli.
4. the temporal course of adaptation to a constant stimulus.

Results of the investigation of the qualitative attributes show that electrical stimulation gives an experience of characteristic pattern, but one which does not differ qualitatively from other cutaneous experiences. The pattern shows two general aspects—a distribution of bright pricks, and a diffuse dull pressure. It might be inferred that two types of receptors are stimulated.

The locus of excitation was tested by comparing the results of electrical stimulation before and after the skin had been cooled with ether to such an extent that sensitivity to pricking by a needle was materially reduced. No clear-cut effect on the electrical experience was observed, which suggests that the effective stimulation may have been in subcutaneous tissue.

Results obtained under 3 and 4 above show that intensities below a certain value adapt out completely; intensities above this value adapt to a certain level and remain at the same apparent intensity indefinitely. Curves showing the relation between intensities of very weak stimuli and times for complete adaptation, and curves showing the process of adaptation to a constant stimulus as measured by the change in the limen, are presented.

[3]Data on which Figure 5 is based appear in Table 3 of the appendix.

Appendix

TABLE 1

Time for Complete Adaptation at Various Intensities

N		*D*		*C*	
Intensity in m.a.	Time	Intensity in m.a.	Time	Intensity in m.a.	Time
.3	30.3	.3	0	.2	0
.27	22.9	.3	1.5	.29	34.0
.28	34.3	.3	1.2	.3	29.5
.32	41.4	.33	10.3	.3	11.0
.32	48.2	.37	5.5	.31	43.3
.35	36.7	.37	29.1	.31	1′ 49.0
.35	38.2	.40	45.8	.32	2′ 43.1
.47	2′ 24.4	.42	51.4	.32	23.9
.40	1′ 58.1	.42	26.7	.34	39.2
.41	2′ 66.5	.43	4.5	.35	31.6
.41	22.3	.45	41.6	.38	1′ 11.3
.46	30.5	.50	52.6	.44	1′ 3.5
.47	36.7	.51	22.1	.48	1′ 58.7
.48	40.6	.56	1′ 33.9	.51	2′ 48.4
.48	58.3	.56	1′ 50.3	.53	8′ 47.2
.49	36.0	.57	1′ 5.0	.53	6′ 49.0
.50	41.5	.57	3′ 14.0	.56	9′ 50.5
.50	49.1	.62	6′ 5.8	.57	19′ 9.0
.51	1′ 3.3	.65	8′ 45.9	.57	9′ 46.5
.51	41.2	.66	8′ 45.9	.58	9′ 38.5
.51	17.5	.66	3′ 41.0		
.51	31.0	.71	8′ 47.1		
.52	56.6				
.52	1′ 25.6				
.53	1′ 4.7				
.54	1′ 0.2				
.60	3′ 52.3				
.61	1′ 30.3				
.63	1′ 2.9				
.71	3′ 13.4				
.73	6′ 4.1				
.74	5′ 54.3				
.78	10′ 6.6				
.82	12′ 25.0				
.85	3′ 44.7				
.88	12′ 2.0				

TABLE 2*

VALUE OF CONSTANT STIMULI IN MILLIAMPERES USED AFTER DIFFERENT PERIODS OF ADAPTATION, AND THE PERCENTAGE OF POSITIVE REPORTS FOR EACH

Observer *C*

10″		30″		60″	
m.a.	%	m.a.	%	m.a.	%
.48	100	.53	100	.60	100
.43	80	.48	80	.55	70
.38	70	.43	60	.50	50
.33	30	.38	40	.45	60
.28	20	.33	20	.40	20
50% value .355 m.a.		50% value .405 m.a.		50% value .475 m.a.	

*Value of limen in unadapted condition is approximately .27 m.a.

TABLE 3*

VALUES OF CONSTANT STIMULI IN MILLIAMPERES USED AFTER DIFFERENT PERIODS OF ADAPTATION, AND THE PERCENTAGE OF POSITIVE REPORTS FOR EACH

Observer *J*

4″		7″		20″		50″	
m.a.	%	m.a.	%	m.a.	%	m.a.	%
.30	90	.35	90	.33	70	.35	50
.28	40	.30	40	.28	10	.30	10
50% value .284 m.a.		50% value .310 m.a.		50% value .313 m.a.		50% value .350 m.a.	

*Value of limen in unadapted condition is approximately .14 m.a.

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LES ASPECTS SENSORIELS DE LA STIMULATION ÉLECTRIQUE

(Résumé)

Le problème ici rapporté a été une enquête exploratrice, désignée pour donner une évaluation et une orientation des phénomènes de la stimulation électrique de la peau comparée avec d'autres formes de stimulation. L'index de la main droite a été stimulé au moyen d'un courant alternatif de 60 cycles à environ un milliampère. L'expérimentateur a régulé l'intensité et la durée de la stimulation. On a étudié les aspects sensoriels suivants des phénomènes: (*a*) les attributs qualitatifs de l'expérience, (*b*) le locus de l'excitation par stimulation électrique, (*c*) la relation entre l'intensité du stimulus et le temps de l'adaptation complète pour les stimuli très faibles, et (*d*) le cours temporel de l'adaptation à un stimulus constant. Deux observateurs entraînes et deux non entraînés ont rapporté les attributs qualitatifs. Les résultats montrent que la stimulation électrique donne une expérience de type caractéristique, mais non autrement différenciée d'autres expériences cutanées. La forme montre deux aspects généraux—une distribution de pointures claires et une pression lourde diffuse. On pourrait penser que deux types de récepteurs sont stimulés. On a testé le locus de l'excitation au moyen de comparer les résultats de la stimulation électrique avant et après que la peau avait été rendue froide par l'éther de sorte que la sensitivité aux pointures d'une aiguille a été beaucoup réduite. On n'a observé nul effet sur l'expérience électrique, ce qui suggère que la stimulation effective a été dans le tissu sous-cutané. Les résultats obtenus sous (*c*) et (*d*) ci-dessus montrent que les intensités au-dessous d'une certaine valeur disparaissent complètement par l'adaptation; les intensités au-dessus de cette valeur s'adaptent à un certain niveau et restent toujours à la méme intensité apparente. On montre des courbes montrant la relation entre l'intensité des stimuli très faibles et la durée de l'adaptation complète, et des courbes montrant le processus de l'adaptation à un stimulus constant mesuré par le changement dans le seuil. Bref, on a trouvé que la stimulation électrique du doigt dans ces conditions donne une expérience qui n'est pas qualitativement unique, qui peut avoir sa base dans le tissue sous-cutané et qui subit un processus très défini d'adaptation.

CROOK

DIE ELEKTRISCHE REIZUNG IN SENSORISCHER HINSICHT

(Referat)

Die hier besprochene Aufgabe war eine erforschende Untersuchung, worin man beabsichtete, eine Erwertung und Orientierung zu liefern, über das Phänomen der elektrischen Reizung der Haut, im Vergleich mit anderen Formen der Reizung. Der Zeigefinger der rechten Hand wurde gereizt mittels eines 60-kreisigen (60-cycle) alternierenden elektrischen Stromes von ungefähr einem Ampere. Die Intensität und die Zeit der Reizung wurden von dem Versuchsleiter geregelt. Es wurden folgende sensorische Qualitäten der Phänomene untersucht: (*a*) die qualitativen Eigenschaften der Erfahrung, (*b*) der geometrische Ort (locus) der Reizung durch elektrischen Strom, (*c*) das Verhältnis zwischen der Intensität des Reizes und die zur vollständigen Anpassung an sehr schwache Reize nötige Zeit, und, (*d*) der zeitliche Verlauf (temporal course) der Anpassung an einen gleichbleibenden (constant) Reiz. Zwei geübte und zwei ungeübte Versuchspersonen gaben über die qualitativen Eigenschaften Bescheid. Die Resultate erwiesen,

dass die elektrische Reizung zwar eine Erfahrung mit karakteristischer Gestaltung (pattern) verursacht, dass diese Erfahrung sich aber nicht weiter von anderen kutanen Empfindungen unterscheidet. Die Gestaltung (pattern) dieser Erfahrung hat zwei allgemeine Züge—eine Verteilung (distribution) von hellen (bright) Stachelempfindungen (pricks) und eine diffuse, dumpfe Empfindung des Druckes. Mann könnte hieraus schliessen, dass zwei Arten von Rezeptoren (receptors) gereizt worden sind. Der geometrische Ort (locus) der Reizung wurde erforscht indem man Vergleichungen machte zwischen den Resultaten der elektrischen Reizung vor und nach Abkühlung der Haut durch Ether bis die Empfindlichkeit dem Nadelstich gegenüber wesentlich herabgesetzt worden war. Es zeigte sich kein Einfluss auf die durch Elektrizität verursachte Erfahrung. Dieser Befund weist darauf hin, dass die wirksame Reizung in sub-kutanem Gewebe stattfand. Die in Bezug auf die ohen unter *c* und *d* erwähten Fragen erhaltenen Befunde erweisen, dass Intensitäten, die unter einen gewissen Wert fallen, durch Anpassung vollständig verschwinden. Intensitäten, die höher liegen, als dieser Wert kann man sich bis zu einem gewissen Grade (level) anpassen, und diese Intensitäten bleiben dann unbestimmt lange auf der selben scheinbaren Höhe. Der Verfasser legt Kurven vor, welche das Verhältnis zwischen der Intensität von sehr schwachen Reizen und die zur vollständigen Anpassung nötige Zeit darstellen und auch Kurven die den Vorgang der Anpassung an einen konstantbleibenden, an Aenderungen der Reizschwelle gemessenen Reiz darstellen. Zusammenfassend wird konstatiert, dass elektrische Reizung des Fingers unter diesen Umständen eine Erfahrung verursacht, welche qualitativ nicht einzigartig ist, welche seine Lage vielleicht in sub-kutanem Gewebe hat, und welche eine ganz bestimmten Prozess der Anpassung durchmacht.

CROOK

FACTORS AFFECTING THE RELIABILITY OF THE MAZE: A COMPARISON OF THE HIGH-RELIEF FINGER MAZE AND AN IMPROVED FORM OF THE STYLUS MAZE*

From Ohio University

THURMAN C. SCOTT AND B. B. NELSON

If one wishes to obtain reliable results in maze work, he must first of all be careful in regard to the pattern and features of his maze. Secondly, he must be careful in the handling and "breaking in" of his subjects. The maze can be used either as a measure of emotional instability (2) or as an instrument for measuring the learning function, and too frequently in the past when it was supposedly measuring learning it has also been measuring emotional instability, and has, of course, been inaccurate as a measure of learning to the degree that it was measuring the other factor. Tryon (10) has recently published an article in which, running rats on an 18-alley T-maze, he obtained a correlation by the odd-vs.-even method on 2-19 trials, inclusive, of .987. This correlation, which he finds is higher than any obtained in the field of mental tests, he believes due to several factors, two of which are: (*a*) that he used a long reliable maze, and (*b*) that he was very careful in "breaking in" his rats, allowing them 8 days to become adjusted to the maze situation. In the present investigation, which parallels Tryon's work to a certain extent, a 10-alley improved stylus maze was used with human subjects, and a correlation of .921 was obtained by the odd-vs.-even method, 1-10 trials, inclusive. This correlation is quite high, considering the fact that the maze has only 10 alleys, and is due, first, to the fact that the maze is reliable in spite of its shortness, and, secondly, to the fact that the experimenters made a special effort to rule out emotional factors. This high correlation, plus the finding of a rather reliable sex difference, are the most important of the findings in an experiment the purpose of which is stated in the following paragraph.

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The object of this investigation was primarily to make a quantitative and qualitative comparison between the wire or so-called high-relief finger maze and an improved form of a stylus maze of identical pattern and size. A second problem, and one closely related to the first, was to test the ability of the human subject to retain and recognize the maze pattern the second day when the two mazes were learned on successive days.

Miles (5) and Husband (4), principally, have written of the superiority of the high-relief finger maze over the stylus maze, but have made no direct comparison. Three of the advantages mentioned by them are as follows: (*a*) the subject has direct cutaneous experience, (*b*) "the finger covers the whole pathway and no possible entrance can be missed, and (*c*) the maze can be traversed more rapidly. They mention other advantages which do not concern us here. These three, however, will be referred to later in connection with the present comparison.

Nyswander (6) has made a statistical comparison between the high-relief finger maze and the old-form stylus maze, a comparison which is quite unfavorable to the stylus maze. She finds the finger maze much less difficult. She also says that the finger maze is more reliable but offers scant evidence for it. The present comparison finds the two mazes about on a par as far as difficulty is concerned and the stylus maze superior for reliability. Part of this difference, however, between Nyswander's and the present results is due to the new features of this maze which are described later.

Nyswander's work can be criticized in several respects. In the first place, she used a maze pattern of very low reliability. Some form of the "T" pattern has proven most reliable (9), but Nyswander used one of Stoelting's stock patterns which has blinds leading off haphazardly, that is, some of them lead straight ahead and some off to one side of the correct path. The chances of making or not making an error at each intersection are not equal in such a maze. In the second place, her groups of subjects are too small to give reliable results, two of the groups having 22 and 26 subjects each. In the third place, she leaves out the records of four subjects who "failed to meet the criterion for learning after an average of 69.5 minutes of actual running time in the maze," yet incorporates in the same group the records of two subjects who exceeded this time, one requiring 75.52 minutes and the other 84.77 minutes of actual running time. Two questions naturally arise in connection with these four

subjects whose records were omitted: first, what would they have done had they been given 84.77 minutes each; and, second, why were they not given that much time. It might be added, however, that these four subjects failed to learn a stylus maze and, had their records been included, the difference between the two mazes would have been greater than it is. In the fourth place, it seems that the experimenters were students and that there were many of them, probably as many as there were subjects. It is true that the experimenter had very little to do in this situation, yet one can be by no means sure that the results were not affected by this variable. In the fifth place, occasionally as many as three pairs of students were working in the "special room" at once. That is, three mazes were occasionally employed simultaneously in the same room, and the presence of other individuals in the maze room is always a factor. In the sixth and last place, Nyswander's interpretation of some of her results is not corroborated by the present findings. For instance, she says that the low reliability of her finger maze "B" is due to the fact that it is too easy. The present writers have used a finger maze which required more trials but fewer errors and less time, yet shows much higher reliability. Further discussion of this point will be given later.

Apparatus and Subjects

The pattern of the two mazes is shown in Figure 1. The finger maze was constructed of No. 13 copper wire. The stylus maze, which has already been described elsewhere, was made by "milling out" the 1/4-in. grooves in a seasoned cherry board. All the horizontal grooves were 1/4 in. in depth. Each vertical groove, regardless of whether a part of the correct path or of a blind alley sloped from a depth of 1/4 in. at the start to a depth of 1/8 in. at the top, thus causing a 1/8 in. "drop-off" into the next horizontal groove. These "drop-offs" prevented retracing. The stylus used in this maze was so constructed that the rounded and enlarged head fit snugly in the 1/4-in. groove. This prevented any feeling of being lost or of having a desire to press against one side of the groove. It enabled the subject to be much freer and bolder in his movements. In fact, he felt just as free and secure, if not more so, than the wire-maze subject who, according to Husband, cannot possibly miss, being aware of every entrance. The two mazes were also identical in size, the vertical lines being 1 in. and the horizontal 4 ins. in length.

The usual blindfold technique was not used. This technique is faulty in two respects. In the first place, it does not always blind

the subject perfectly, and, in the second place, if worn very long it may become painful and prove a distraction. A platform was built over the area occupied by the maze on the table (8). This platform had an open side toward the subject so that he could observe the maze running. One end of a piece of denim was fastened to this platform and the other end was fastened around the subject's neck as an apron. This proved to be a very effective blindfold as far as the maze was concerned and was never uncomfortable.

Experimental Procedure

The authors are thoroughly in accord with Tryon's statement that more reliable results can be obtained with subjects who are thoroughly "broken in." Tryon allowed his rats to take short runs on a practice maze for 8 days before trying them on the test maze. This "breaking in" is possibly just as important in the case of human subjects, and in a previous experiment (8, pp. 168-169) the first day was devoted to familiarizing the subjects with mazes. In the present case, however, it was thought possible to accomplish this by giving the first 10 minutes of the period to an explanation of what the subject was to

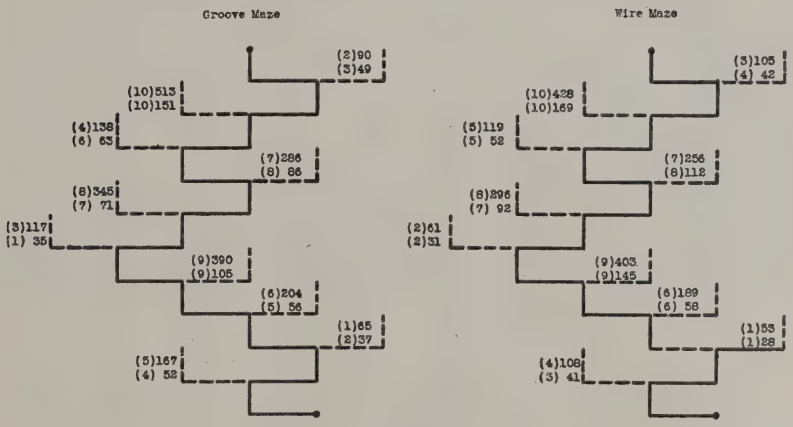

FIGURE 1

Patterns of the Mazes and the Number of Errors Made in Each Alley

The total number of errors made in each alley the first day by one group and the total number made on the second day by the other group are indicated by the upper and lower numbers, respectively, just to the left of the alleys. The numbers in parenthesis indicate the rank of the alleys from easiest to most difficult.

do. During this time it was explained that learning a maze was not a test of one's intelligence and that one should attack the maze as he would an intriguing puzzle. The back side of the maze was shown to the subject and it was pointed out that from the starting point near one edge of the board he would gradually work across to the goal near the other edge. The subject was also told that he should not be disappointed if he did not solve the maze immediately and that patience was a valuable trait for a maze runner. At this point he was asked to read the following set of instructions:

1. The subject is to strive for an early mastery of the maze, that is, to learn the true path from the entrance to the goal as quickly as possible. Accuracy, however, should not be sacrificed for speed.
2. The maze will be considered learned when the subject has made five runs in succession without error.
3. An error is defined as a turn from the true path into a blind alley.
4. Time will be counted from the starting signal until the exit is reached.
5. An interval of one minute will be given between trials.
6. Subjects are not to discuss the mazes with one another.

If the subject had no questions, the maze was put in position and the subject was asked to run his hand around the maze board in order to verify his position. He was also allowed to examine either with finger or stylus, depending upon which maze he was learning, the first move in the maze up to the first cross section. He was then considered ready to start. All the subjects with the exception of four learned the first maze at one sitting and learned the other maze the next day. The four subjects who did not learn the maze the first day continued to come back each day for a 50- or 60-minute period until they had mastered the maze.

Results for First Day

Fifty subjects, which are designated as Group 1, learned the stylus maze the first day and the wire maze the second day. Fifty subjects, designated as Group 2, learned the wire maze the first day and the groove maze the second day. In Table 1 is found a comparison of these two groups on the maze learned the first day. This comparison is given in terms of trials, errors, and total running time. The first 24 subjects listed in each group are boys and the last 26 are girls.

TABLE 1

TOTAL SCORES IN TRIALS, ERRORS, AND TOTAL RUNNING TIME FOR GROUPS 1 AND 2

Group I Stylus maze				Group 2 Wire maze			
Subject	Trials	Errors	Seconds	Subject	Trials	Errors	Seconds
1	13	27	279	1	40	80	2038
2	8	22	167	2	10	22	598
3	15	24	353	3	7	17	374
4	8	22	155	4	17	40	846
5	21	31	672	5	23	66	841
6	6	24	285	6	7	17	265
7	24	72	645	7	18	32	385
8	11	31	572	8	12	20	413
9	18	50	433	9	11	16	424
10	38	105	1174	10	25	51	754
11	35	70	651	11	15	31	830
12	11	26	539	12	11	39	572
13	11	20	375	(13)	73	178	3265
14	13	30	587	14	6	20	209
15	9	22	272	15	11	22	494
16	12	39	454	16	14	27	708
17	5	18	290	17	13	28	497
18	10	27	359	18	11	25	417
19	6	17	481	19	25	45	754
20	15	40	798	20	26	26	621
21	13	32	435	21	46	85	1754
22	5	6	230	22	4	8	429
23	4	8	387	23	11	26	468
24	18	34	865	24	6	11	323
25	23	46	617	25	8	12	289
26	42	75	415	26	25	65	761
(27)	58	120	1064	27	15	44	757
28	12	23	592	28	25	51	846
29	8	21	248	29	16	42	522
30	30	96	972	30	20	43	1006
31	12	26	373	(31)	60	127	2922
32	30	95	979	32	13	24	637
(33)	65	175	1048	33	8	17	390
34	12	30	603	34	26	60	1099
35	20	58	689	35	10	20	604
36	61	90	1505	36	12	30	732
37	18	43	338	37	14	35	518
38	25	84	1200	38	19	37	867
39	13	30	474	39	13	32	424
40	20	66	873	40	15	41	829
41	14	28	458	41	16	26	766
42	15	26	480	42	8	20	267
43	24	60	525	43	7	11	398
44	42	71	1585	44	17	33	978
45	11	28	489	45	22	47	1592
46	13	19	1217	46	22	63	1176
47	43	107	1342	47	22	59	747

TABLE 1 (*continued*)
TOTAL SCORES IN TRIALS, ERRORS, AND TOTAL RUNNING TIME FOR GROUPS 1 AND 2

Group I Stylus Maze				Group 2 Wire maze			
Subject	Trials	Errors	Seconds	Subject	Trials	Errors	Seconds
48	18	50	841	48	15	34	711
49	18	40	839	49	15	38	605
50	7	13	587	50	37	74	1388
Mean	19.66	46.34	636.22	Mean	18.52	40.34	813.46
S.D.	14.37	33.57	344.82	*S.D.*	13.24	29.67	595.13
No.=48				No.=48			
Mean	17.92	42.13	618.73	Mean	16.73	35.67	718.46
S.D.	11.78	26.43	340.89	*S.D.*	9.00	18.56	375.90

All 50 subjects in each group met the criterion for learning on the first maze. There were two subjects, however, in Group 2, subjects 13 and 31, who varied greatly from the error means, and two in Group 1, subjects 27 and 33, who varied to a lesser extent. These four subjects required more than one sitting to learn the maze. Means and *S.D*'s are given both for groups of 50 and for groups of 48.

In Table 2 is found a more direct comparison of these scores, and, while there are differences between the scores obtained with the two mazes, it can readily be seen that these differences are not reliable. The standard score of difference is obtained by dividing the difference between the means by the standard error. This standard score of difference should be as high as three to indicate that a com-

TABLE 2
COMPARISON OF MEANS AND STANDARD DEVIATION OF THE TWO MAZES

	Stylus maze *N*=50		Finger maze *N*=50		Standard Error	Standard score of difference
	Mean	*S.D.*	Mean	*S.D.*		
No. of trials	19.66	14.37	18.52	13.24	2.76	.413
No. of errors	46.34	33.57	40.34	29.67	6.33	.948
No. of seconds	636.22	344.82	813.46	595.13	97.26	1.822
	Stylus maze *N*=48		Finger maze *N*=48		Standard Error	Standard score of difference
	Mean	*S.D.*	Mean	*S.D.*		
No. of trials	17.92	11.78	16.73	9.00	2.14	.556
No. of errors	42.13	26.43	35.67	18.56	4.66	1.386
No. of seconds	618.73	340.89	718.46	375.90	73.24	1.363

pletely reliable difference exists. In this case, however, it does not approach three in any instance.

From this table it can be seen that there are small differences in trials and errors in favor of the finger maze and a greater difference in time in favor of the stylus maze. This time difference is interesting, since it exists in the opposite direction to that of the differences in trials and errors, and is quite contrary to Nyswander's findings and to Miles and Husband's predictions. This superior time record is due to the fact that the stylus moves much more smoothly through the groove than the finger does over the wire, and this snug-fitting stylus point prevents any excess wandering movements which are possible in the old-type stylus situation. It is evident, however, that none of these differences are reliable, even when two extreme cases are thrown out of each group. Nyswander finds complete reliability in all three criteria, all the differences existing in favor of the wire maze.

Reliability Coefficients

Reliability coefficients were computed in terms of errors for the two mazes. The two methods used by Nyswander, odd-vs.-even, and first-half-vs.-second-half, were used. Only the first ten trials were included since several subjects learned the mazes in this number or fewer trials. These reliability coefficients are given in Table 3.

It is evident that the groove maze is slightly superior to the finger maze with respect to the reliability of the learning scores. What this superiority is due to is difficult to say. It might be argued that the wire maze is easier than the groove maze and therefore is less reliable. This difference in difficulty, however, is too slight to be reliable, exists only in terms of trials and errors, and should be offset to a certain extent by the greater difference in difficulty in the oppo-

TABLE 3

Reliability Coefficients Computed by Two Methods for Both Mazes

	Stylus maze	Finger maze
Odd vs. even scores	.921±.014	.852±.026
1st half of scores vs. 2nd half	.750±.041	.647±.055

The reliability coefficients are corrected for halving the data by the Spearman-Brown formula: $R = \frac{r}{1 + r}$.

site direction in the time scores. It might be argued more safely that this superiority of the groove maze is due to the differences in the cues obtained in the two cases. In the first place, the subjects were much bolder and freer in their movements in the groove than on the wire maze, the reason being that there was no danger of slipping out of the groove, whereas there was danger of slipping off of the wire at turns and at the end of alleys. Frequently, a wire-maze subject had difficulty in getting his finger to slip along smoothly on the wire. If the finger was damp from perspiration there was a tendency for it to stick occasionally and hop along rather than move smoothly. Occasionally, also, and in spite of instructions to the contrary, a subject would bear down so hard on the wire that he would numb the end of his finger and would not be able to feel the cross wires at intersections. Such a subject, having lost most of his cutaneous cues, had to depend largely on kinaesthetic cues and was necessarily disturbed to a certain extent by such shifts in method. These subjects when failing to feel the cross wire would start retracing, and even though they were checked immediately, the disturbance was too great for them to profit much from that particular trial. These conditions, then, introduced in the wire-maze scores a certain amount of uncertainty which would tend to make them less reliable. On the other hand, it can be said that the groove-maze subjects had almost no cutaneous cues at all, and it is true that they suffered somewhat from this lack. Their kinaesthetic cues, however, were much better than those of the wire-maze subjects, and the separate moves were knit into a more unified pattern. The auditory cues, produced by the clicking of the stylus against the walls at the turns, were also an aid in the groove maze and helped the subjects develop a rhythmical movement. No such movement was noted in the case of the wire-maze subjects.

One other point should be mentioned in connection with these reliability coefficients. Nyswander gets rather low coefficients (odd-vs-even=.557±.059; first-half-vs.-second-half=.104±.102) with her Finger Maze B, and, as has already been mentioned, argues that it is due largely to the fact that the maze is very easy, in fact, too easy. That the easiness of the maze is not the main factor can be seen from the comparison in Table 4. Two columns are given under each of the present mazes, the first being for 50 and the second for 48 subjects.

On the basis of trials, Nyswander's maze does appear to be easier

TABLE 4

	Nyswander's Finger Maze B	Present Finger maze		Present Stylus maze	
		N=50	N=48	N=50	N=48
No. of trials	12.88	18.52	16.73	19.66	17.92
No. of errors	42.98	40.34	35.67	46.34	42.13
No. of seconds	799.20	813.46	718.46	636.22	618.73

than those of the present authors', but on the basis of errors and time it is about on a par. The number of errors is the most important criterion of ease or difficulty, since the reliability coefficients are calculated in terms of errors. This simply means that the pattern of the maze, a factor which Nyswander mentions, and not the ease or difficulty of the maze, is the most important factor here in determining the reliability coefficients.

Relative Difficulty of Alleys

The relative difficulty of the alleys in the two mazes is presented in Figure 1. The upper number just to the left of each alley is the total number of errors made in that alley by the 50 subjects learning that particular maze the first day. The number in parentheses still further to the left represents the rank of that alley from easiest to most difficult in comparison with the other nine alleys of the maze. If the ranks of the respective alleys in the two mazes are compared for first-day scores, it is found that six of them occupy the same position and the other four differ by only one position. If a correlation coefficient is computed between the alleys of the two mazes either by the rank-difference or the product-moment methods, it is found to be .976. This figure indicates that there is a high degree of similiarity in difficulty of alleys of the two mazes and that the two mazes are measuring the learning function in the same way.

TABLE 5

Comparison of Means for the Two Mazes on the Second Day

	Wire maze Group 1 N=50	Stylus maze Group 2 N=50
No. of trials	8.60	7.50
No. of errors	15.40	14.10
No. of seconds	337.36	253.94

Results for Second Day

Group 1 which learned the stylus maze the first day learned the wire maze the second day without being told that the pattern was the same. Group 2, under similar conditions, learned the stylus maze the second day. In Table 5 is found a comparison of these two groups on their second maze. As in Tables 1 and 2, this comparison is given in terms of trials, errors, and total running time. The findings here are similar to, and corroborate, those offered in Tables 1 and 2.

On the basis of trials and errors the stylus maze seems slightly easier the second day than the wire maze. This difference, however, is too small to be of any consequence. The time difference, though, in favor of the stylus maze is greater and is of more importance. The time difference between the two mazes is in agreement with that found in the scores for the first day, but the slight trial-and-error differences are in disagreement with those of the first day. None of the first-day differences, however, were anything like reliable, so this slight disagreement is not surprising. No standard deviations are given in connection with this table, since the factor of recognition of maze patterns cuts across and spoils an accurate statistical comparison.

Recognition of Maze Pattern

A comparison of Tables 2 and 5 shows that the subjects have improved greatly in maze-running ability by the second day. The scores are from 1/3 to 1/2 their original size. This great saving the second day is due, in the first place, partly to practice effect. Learning one maze, as a rule, enables one to learn another maze more easily, regardless of the fact that there may be very little similarity between their patterns. In the second place, this saving is due to the retention of the pattern from the first to the second day. This retention may be on the level of awareness or it may function below this level, that is, "subconsciously." In the first condition, we have either complete or partial recognition of the pattern. In Table 6 is found a comparison of Groups 1 and 2 when divided into the following subgroups: (*a*) those unaware of any similarity of pattern, (*b*) those who noticed some similarity; and (*c*) those who recognized pattern as same.

In the first place, it should be noticed that there is no great differ-

TABLE 6

COMPARISON OF SUBGROUPS OF GROUPS 1 AND 2 IN TERMS OF MEANS

	Group 1—wire maze (2nd day)				Group 2—stylus maze (2nd day)			
	No.	Trials	Errors	Sec.	No.	Trials	Errors	Sec.
a. Unaware	23	12.35	23.65	496.78	19	9.84	17.79	304.37
b. Some sim.	11	5.27	8.18	245.09	14	7.71	15.50	298.79
c. Recognized	16	5.38	8.50	171.69	17	4.65	8.88	160.79

ence between Groups 1 and 2 in regard to the number of subjects who fall into the different subgroups. There were four more in Group 1, who were totally unaware of any similarity, than in Group 2, but this is merely a chance difference. The stylus maze seemed somewhat easier to Group 2 than the wire maze did to Group 1 on the second day, and this shows up in all the subgroups except the second, the one noting some similarity. This difference between these two subgroups in Groups 1 and 2, however, may be due to the fact that this subgroup covers a rather wide range of knowledge in regard to the maze. All subjects were included in this group, from those who were aware of any similarity whatsoever to those who were practically sure that as many as nine of the ten moves in the mazes were identical, but were not sure of the tenth. One should not be surprised at a certain amount of difference between two small groups when the conditions could be so varied.

The relative difficulty of the alleys in the two mazes for the second day is also shown in Figure 1. The lower number just to the left of each alley is the total number of errors made in that alley by the 50 subjects learning that particular maze the second day. The number in parentheses still further to the left indicates the rank of that alley from easiest to most difficult. If these ranks of the alleys in the two mazes are compared, it is found that four hold the same positions and the other six differ by only one position. The correlation coefficient which is obtained from this comparison by the rank-difference method is .964. This verifies the high correlation which was obtained from such a comparison in the first day's records.

It might be said, in summarizing the data on the mazes learned the second day, that there is no evidence for believing that there is a reliable difference between the two mazes. There is almost no difference as far as retention and recognition of pattern is concerned. The two mazes seem equally easy on the basis of trial-and-error

scores. The groove maze, however, seems to be about a third more economical of time. This time difference is not important in itself, except that it does corroborate the time difference found in the first day's scores. The relative difficulty of the alleys in the two mazes on the second day is practically the same as that found the first day.

Learning Curves

In Figure 2 is found a comparison of these two mazes for both days in terms of their error and time learning curves. These time curves are not particularly reliable on the last two or three trials, since some subjects had already completed the five perfect runs in succession before reaching the last few trials. In such cases it was assumed that these subjects would have continued making perfect runs using the smallest amount of time which they had required on any of the five perfect runs. In actuality, they probably would have used less time, so this assumption may have flattened the curves out slightly on the last few trials. The error learning curves show once

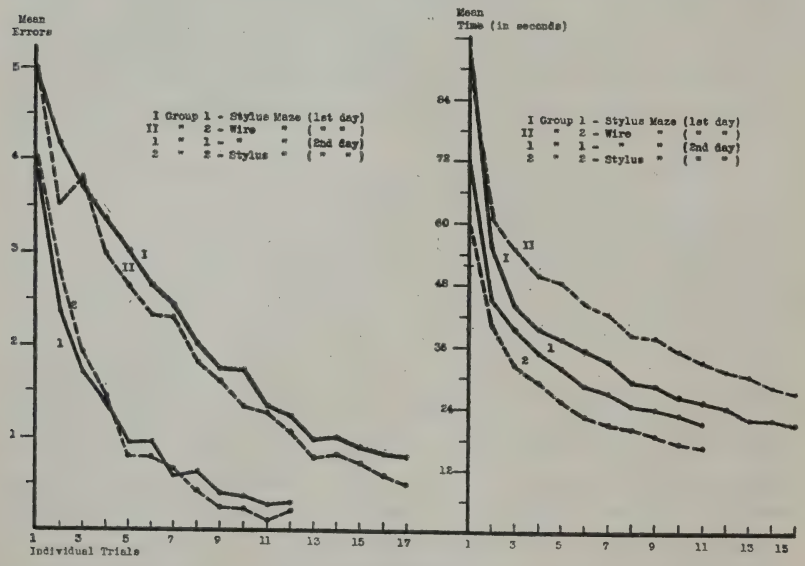

FIGURE 2

Comparison of the Two Mazes in Terms of Error and Time Curves for the Two Days

The error curves show a slight difference in favor of the wire maze, but the time curves show a greater difference in favor of the stylus maze. None of these differences are reliable.

more that there is very little difference between these two mazes on either the first or the second day. There is a slightly greater difference in the time scores and this difference exists in favor of the stylus maze. This time difference is particularly noticeable if the improvement which Group 1 made from the first to the second day is compared with the improvement which Group 2 made from the first to the second day. Group 2 changed from the wire maze the first day to the stylus maze the second day, and shows a great saving in time. Group 1, on the other hand, made the opposite change and shows much less saving in time. The two time curves for Group 1, then, fall between those of Group 2. This explains why the gap between the time learning curves of the first day, on the one hand, and the curves of the second day, on the other, is not as noticeable as that existing between these particular pairs of error curves.

Sex Difference

As has already been stated, there were 24 boys and 26 girls in each group. Since the differences between the two mazes has not been found to be reliable, and particularly since each sex is equally divided between the two groups, and the number of each sex is almost the same, one can justifiably group the subjects according to sex. In Table 7 this sex comparison is made.

In connection with Tables 1 and 2, it was mentioned that there

TABLE 7

Comparison of Sexes

	48 boys		52 girls		Standard error	Standard score of difference
	Means	*S.D.*	Means	*S.D.*		
No. of trials	16.15	12.77	21.81	14.22	2.69	2.10
No. of errors	36.02	29.22	50.10	32.63	6.18	2.28
No. of seconds	618.50	520.75	822.96	447.89	97.50	2.10

TABLE 8

	47 boys		49 girls		Standard error	Standard score of difference
	Means	*S.D.*	Means	*S.D.*		
No. of trials	14.94	9.80	19.41	10.69	2.09	2.14
No. of errors	33.00	20.83	44.55	23.61	4.54	2.54
No. of seconds	562.19	353.22	770.61	343.52	70.86	2.96

were four subjects, one boy and three girls, who varied greatly from the error means. If these four subjects are not included we get a comparison as set forth in Table 8.

This boy, case No. 13 in Group 2, was the extreme boy not only on the basis of the error score, but also on trial and time scores. The extreme girl, case No. 33 in Group 1, was extreme on trial-and-error scores, but there were 11 other girls who scored higher on time than she did. If these two cases, the one boy and the one girl, who were quite extreme, are omitted, we have the comparison as set forth in Table 9.

It is evident from Tables 7, 8, and 9 that there are sex differences in favor of the boys. These sex differences, while not completely reliable, are very nearly so. The lowest standard score of difference is 2.10, which indicates that the chances are 98 in 100 that the trial and the time differences between these scores are greater than zero. The highest is 3.15, a figure which indicates complete reliability. It should be noted that these standard scores of difference are much higher than those found in comparing the two mazes. They also measure the reliability of differences, all of which exist in the same direction. The reliability of the differences existing between the two mazes, while low in itself, is made even more questionable by the fact that the time difference exists in the opposite direction to that of the trial-and-error differences.

Trial, error, and time scores for the second day agree with those

TABLE 9

	47 boys		51 girls		Standard error	Standard score of difference
	Means	*S.D.*	Means	*S.D.*		
No. of trials	14.94	9.80	20.96	13.07	2.32	2.59
No. of errors	33.00	20.83	47.65	27.28	4.94	2.97
No. of seconds	562.19	353.22	818.57	451.14	81.51	3.15

TABLE 10

COMPARISON OF MEANS FOR THE SIX GROUPS ON SECOND DAY

	Boys	Girls
No. of trials	7.00	8.96
No. of errors	12.06	17.25
No. of seconds	246.15	341.35

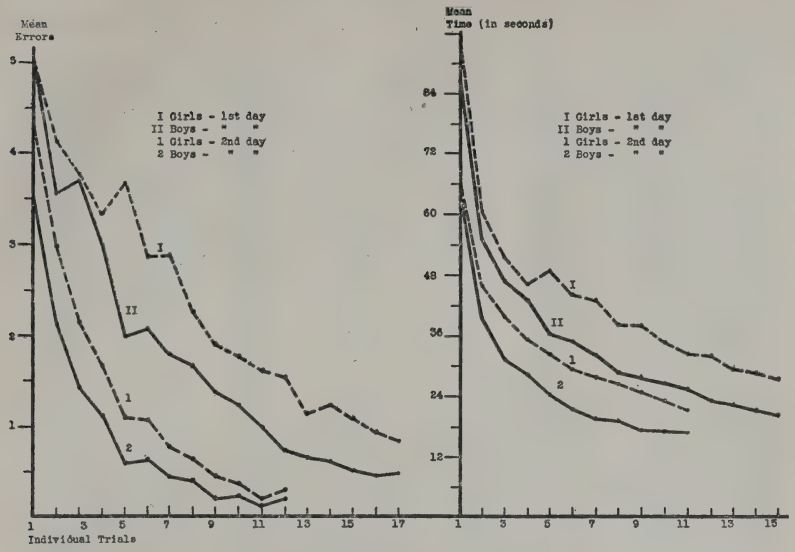
Mean Errors
I Girls - 1st day
II Boys - " "
1 Girls - 2nd day
2 Boys - " "
Individual Trials
Mean Time (in seconds)
I Girls - 1st day
II Boys - " "
1 Girls - 2nd day
2 Boys - " "

FIGURE 3

COMPARISON OF SEXES IN TERMS OF ERROR AND TIME CURVES FOR THE TWO DAYS

The boys are superior throughout and, while the differences are not completely reliable, they are much more nearly so than those existing between the two mazes.

for the first day and offer further evidence of a sex difference in favor of the boys in maze-running ability. No standard deviations are given here for the same reason that they were omitted in Table 5.

In Figure 3 is found a comparison of the sexes for both days in terms of their error and time learning curves. These time curves are not particularly reliable on the last two or three trials for the same reason that those in Figure 2 are not particularly reliable. The difference between the sexes seems more evident in the error than in the time curves. It is only slightly more so, there being 989 chances out of 1000 that there is a real error difference, and 980 out of 1000 that there is a real time difference. Most of this seemingly greater evidence of an error difference is due to the fact that the error curves are drawn on a much larger scale than the time curves. The sex difference in the time scores may also be slightly obscured by the fact that it is cut across by a time difference between the two mazes. The time difference between the sexes is about as nearly completely

reliable as the error difference, even though the two are not equally evident.

What this superiority of the boys in maze-running ability is due to is not clear. One element in the situation may have been the fact that the two experimenters were men and the girls were somewhat less at ease in their presence than the boys. Another factor, and one which is probably more important, is the fact that girls, as a rule, take experiments more seriously than boys and try harder. That is, their attitude is more tense. This tenseness is supposed to aid in some experiments, and has been advanced as one of the reasons why girls are superior to boys in remembering names and faces in a photographic experiment.[1] Bills (3) has also found that "tension" is an aid in maze learning. He found that the subjects who had most tension learned the maze more quickly and made fewer errors. He has nothing to say about the sex of his subjects, and it is quite possible that he had no great range in tension. Tension, at least, it is safe in assuming, did not approach emotional instability. Concentration and seriousness of purpose are characteristics which are necessary in the maze runner, but tension or tenseness beyond a certain point become a disadvantage. A subject can easily try too hard—a statement which is just as true of the maze runner as of the golfer or tennis player. Such a subject, as a rule, has a "self" rather than a "problem" attitude toward the maze and is visibly affected by his errors. In fact, these subjects have not been sufficiently "broken in," and are showing evidence of some emotional upset. A third factor which is frequently advanced as an explanation of such a sex difference is the fact that boys and girls have developed somewhat different patterns of overt motor behavior, in fact, that the boys are more fully developed than the girls. The present writers, however, are not inclined to give much weight to this argument, since the type and scope of movements necessary to run a small maze of this sort can be developed as easily and as accurately by cutting out paper dolls as by playing ball. This factor, if it is a factor, is at least secondary to the difference in the emotional response of the two sexes to the maze situation. In any case, however, this superiority of the boys in maze-running ability is not "due solely to sex" (1), but is due most probably to some difference in the social training of the two sexes.

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[1]This experiment, conducted by Virginia R. Hathaway at Ohio University under the direction of the first of the writers, has not been published.

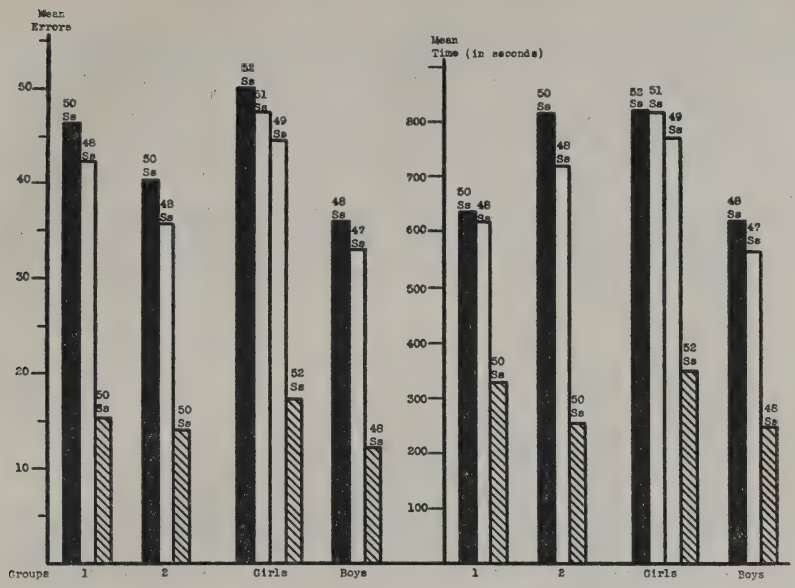

FIGURE 4

COMPARISON OF THE DIFFERENCES BETWEEN THE MAZES (GROUPS I AND II) AND THE DIFFERENCES BETWEEN THE SEXES

This figure shows that the differences between the mazes are not as great as those between the sexes. The black columns and the white columns represent the first day's results, while the checked columns represent the second day's results.

Groups 1 and 2 and the two groups of boys and girls are summarized graphically for mean errors and mean time in Figure 4. The last column in each group represents the mean of the errors or time on the second day. All the other columns represent the records of the first day. First-day columns are offered not only for the full group but for the group minus the few extreme individuals. The number of the individuals included is given at the head of each column. The designation of the group is given underneath each set of columns. This figure shows somewhat more plainly the facts mentioned in connection with the groups or tables here summarized, namely, that the differences existing between Groups 1 and 2 are not as reliable as the differences existing between the group of boys and the group of girls.

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LES FACTEURS INFLUANT SUR L'EXACTITUDE DU LABYRINTHE; UNE COMPARAISON DU LABYRINTHE À DOIGT DE HAUT RELIEF ET UNE FORME PERFECTIONNÉE DU LABYRINTHE À STYLET

(Résumé)

Cette enquête a eu comme but une comparaison entre le labyrinthe à doigt de haut relief et une forme perfectionnée d'un labyrinthe à stylet. On a construit ce labyrinthe à stylet de sorte qu'on a empêché le retracement. Le stylet a été bien serré au sillon et le sujet n'a pu s'écarter dans le sillon et se croire égaré.

Cent sujets ont appris un labyrinthe à stylet et un labyrinthe à fil de fer de même forme et grandeur. Le groupe 1, composé de 50 sujets, a appris le labyrinthe à stylet le premier jour et le labyrinthe à fil de fer le deuxième jour. Le groupe 2, composé de 50 sujets, a appris le labyrinthe à fil de fer le premier jour et le labyrinthe à stylet le deuxième jour. Dans les deux cas on n'a pas dit aux sujets que les formes étaient pareilles. Il a fallu au groupe 1 un peu plus d'essais et d'erreurs, mais moins de temps, pour apprendre le labyrinthe à stylet qu'il en a fallu au groupe 2 pour apprendre le labyrinthe à fil de fer. Ces différénces, cependant, ont été de très peu de valeur, surtout dans le cas des essais et des erreurs, la différence obtenue entre les moyens étant moins que l'erreur étalon de difference.

La difficulté relative des couloirs respectifs des deux labyrinthes indique qu'ils mesurant la fonction de l'apprentissage de la même manière. La corrélation computée en terms du nombre total des erreurs faites dans les couloirs respectifs des deux labyrinthes est de 0976.

On a computé les coefficients de constance pour les deux labyrinthes en termes des erreurs dans les dix premières épreuves et ils montrent que le labyrinthe à stylet est quelque peu supérieur au labyrinthe à doigt dans l'apprentissage des expériences.

Vingt-quatre sujets dans chaque groupe de 50 ont été garçons, donnant des groupes de sexe de 48 garçons et de 52 filles. Ces deux groupes offrent des différences en faveur des garçons lesquelles ont plus de valeur que celles existant entre les deux groupes, considérés au point de vue du type de labyrinthe. SCOTT ET NELSON

FAKTOREN DIE DIE ZUVERLÄSSIGKEIT DES LABYRINTHES BEEINFLUSSEN. EINE VERGLEICHUNG DES STARK HERVORTRETENDEN FINGERLABYRINTHES MIT EINER VERBESSERTEN FORM DES GRIFFELLABYRINTHES

(Referat)

Es war der Ziel dieser Untersuchung, einen Vergleich zu machen, zwischen dem stark-hervortretenden Fingerlabyrinth (high-relief finger maze) und einer verbesserten Form des Griffellabyrinthes (stylus maze). Dieser Griffellabyrinth war so konstruirt, dass die Wiederbetretung [des schon zurückgelegten Teiles] vermieden wurde. Das Griffellabyrinth war auch so gemacht, dass es eng zusammenhing und es der Versuchsperson nicht möglich war, in der Furche herumzuwandern und das Gefühl des verirrtseins zu haben.

Hundert Versuchspersonen erlernten ein Griffellabyrinth und ein Drahtlabyrinth (wire maze) von identischem Muster und identischer Grösse. Gruppe 1, bestehend aus 50 Vpp., erlernte an dem ersten Tage das Griffel- und an dem zweiten das Drahtlabyrinth. Gruppe 2, ebenfalls aus 50 Vpp. bestehend, erlernte am ersten Tag das Draht- und am zweiten das Griffellabyrinth. In beiden Fällen hat man den Vpp. nicht gesagt, das die Muster gleich waren. Gruppe 1 machte bei dem erlernen des Griffellabyrinthes etwas mehr Versuche und Fehler, brauchte aber im Ganzen weniger Zeit, als Gruppe 2 bei dem Erlernen des Drahtlabyrinthes nötig hatte. Diese Unterschiede waren aber höchst unzuverlässig, besonders in Bezug auf Versuche und Fehler, wobei der erhaltene Unterschied zwischen den mittleren Zahlen (means) geringer war, als der Normalfehler des Unterschiedes.

Die relative Schwierigkeit der respektiven Alleen der zwei Labyrinthen weisen darauf hin, dass sie die Lernfähigkeit auf gleicher Weise messen. Die Korrelation, berechnet in Bezug auf die Gesamtzahl der Fehler die auf den respektiven Alleen der zwei Labyrinthen gemacht wurden, betrug .976.

Die Koeffizienten der Zuverlässigkeit der zwei Labyrinthen wurde berechnet in Bezug auf die Fehler bei den ersten zehn Versuchen und weisen darauf hin, dass sich das Griffellabyrinth zu Lernexperimenten etwas besser eignet als das Fingerlabyrinth.

24 Vpp. aus jeder Gruppe waren Knaben. Im Ganzen gab es also 48 Knaben und 52 Mädchen. Diese Gruppen, nach Geschlecht geordnet, erweisen Unterschiede zu Gunsten der Knaben welche zuverlässiger sind, als die die zwischen den zwei Labyrinthen bestehen. SCOTT UND NELSON

AN EMPIRICAL STUDY OF THE ABILITY TO GENERALIZE*

From the Department of Psychology of the University of Chicago

GEORGE M. PETERSON

INTRODUCTION

In this study we are concerned with a series of problems all involving a general principle which, if discovered and applied in their solution, will give the correct answer. The problems were given to primary and secondary school students with a view to discovering the trend of ability in solving them and the correlation of this ability with age, intelligence, and grade in school. We are also interested in any sex differences that may be present and in the incidence of the ability to state the general principle as we progress through the school grades.

The problems which we gave all involved the general principle of the law of the lever which may be stated: *Any lever is balanced when the moment on one side of the fulcrum is equal to the moment on the other side.* That is, a lever is balanced when the weight times the distance on one side is equal to the weight times the distance on the other.

APPARATUS

Our apparatus was a balance or lever of the first class, the arms of which were made of bakelite, each 9½ inches in length. Four hooks were placed at 2-inch intervals on the lower side of each arm. Running out consecutively from the fulcrum, and above each hook were the numbers 1, 2, 3, 4, painted in white. These were easily visible from the farthest distance in a normal-sized classroom.

Ten weights made of bakelite, one inch in diameter and weighing 1, 2, 3, 4, 5, 6, 8, 9, 10, and 12 ounces, respectively, were used. Thus the 12-ounce weight was about twelve times the length of the one-ounce weight. The appropriate numbers were painted in white on each weight.

*Recommended for publication by Harvey A. Carr, accepted by Carl Murchison of the Editorial Board, and received in the Editorial Office, September 3, 1931.

Information Concerning the Source of Data

Our data were all collected in the Chicago Public Schools. Since they do not have intelligence test records it was necessary for us to give our own test to obtain these. We used the Terman Group Test of Mental Ability for Grades 7 to 12, Form A. All testing was done in the afternoon. The intelligence test was given on the first afternoon in the case of each group and the weight problems were given on the second afternoon. Where two groups were obtained from the same school, which occurred in five instances, the intelligence tests were given successively on the same afternoon and the weight problems successively on the next afternoon. In no case did these groups in the primary schools have recess together, hence they could not communicate regarding the nature of the work. The only secondary school, Fenger High School, to contribute two groups had a five-minute passing period between the two test periods, but, since the groups were at the opposite ends of the building and neither knew of the other's participation in the work, we feel sure that the results were not affected by any communication between groups.

Table 1 gives a list of the schools, grades, and number of subjects used in this study.

TABLE 1

Primary Schools	Grade	Number of subjects
Cornell	5B	42
Bass	6B	42
Cornell	6A	43
Beale	7B	40
Burnside	7A	38
Bass	$8B_1$	35
Beale	$8B_2$	42
Burnside	8A	41
Secondary Schools	**Grade**	**Number of subjects**
Hyde Park	9B	39
Lindbloom	9A	32
Fenger	10B	31
Parker	10B	38
Fenger	10A	30
Tilden	11B	26
Englewood	11A	31
Bowen	12B	27

Our age and grade records were obtained from the intelligence test blanks which the students filled out. Age was computed in months. In a few instances the age did not check with the date of birth listed and in these cases the age record was always used, since it was considered that a child is more likely to know how old he is rather than in what year he was born.

Procedure

In giving the intelligence tests Terman's directions were followed.

In giving the weight problems the following procedure was followed:

After the apparatus was set up and papers passed for the students to write their answers on, the experimenter said:

> "Write your name on the paper you have just received. Notice that this paper has two columns on it, one headed 'problem number' and the other headed 'answer.' You are going to have twenty problems to solve. Be sure to get your answer in the right place.
>
> "All of these problems involve a general principle. That is, there is a method or way of doing them so that you can always get the problems right. It is up to you to discover what this way is as we go along. Then, after we have finished, I am going to ask you to write out this principle, that is, write out how you worked the problems. Now, I am going to show you a problem, then ask you to do one, then show you another, and then have you do another. It is when I show you that you must try to discover how to do them.
>
> "I have here a balance" (at this point I tipped it back and forth) "with hooks under each of these numbers as you go out from the center. These hooks are at equal distances as we go out. Now here is a set of weights" (here I held each one up in order). "Here is a 1-ounce weight with the number 1 on it, a 2-ounce weight with the number 2 on it, etc., up to a 12-ounce weight with the number 12 on it.
>
> "Now if I put an 8-ounce weight on hook 3" (I did this) "and wanted to balance it with a 12-ounce weight, I would put it on hook 2." (I did this.) "If I had put it on hook 1, it would not have been heavy enough." (I did this.) "If I had put it on hook 3 or 4 it would have been too heavy." (I did both.) "But on 2 it just balances." (I did this again.) "It is up to you to find out why this is, and when you know you can always get every problem right.

"Here is your first problem. If you had a 1-ounce weight on 4" (I put the 1 on 4), "where would you put a 2-ounce weight to balance it?" (I held up the 2 before them.) "Write your answer after problem No. 1 on the paper. That is, write down the number of the hook you think it should go on. Don't hurry. Take your time and figure carefully.

"Now I will show you another. If I had a 12-ounce weight on 1 and wanted to balance it with a 3-ounce weight, I would put it on 4." (I did this.) "Why is this? When you find out why you can get every problem right.

"Here is your second problem. If you had a 2-ounce weight on 3" (I put the 2 on 3), "where would you put a 6 to balance it?" (I held up the 6.) "Write your answer after problem No. 2."

This procedure continued until 20 problems were demonstrated and 20 problems were given to solve. In all, it took about 30 minutes to give the test, although no time limit was set. A new problem was not started until everyone had all the time he wanted on the old one.

The following order was given for the problems:

1. Demonstrated 8 on 3 12 on 2
   1. Given to solve 1 on 4 2 on (2)
2. Demonstrated 12 on 1 4 on 3
   2. Given to solve 2 on 3 6 on (1)
3. Demonstrated 12 on 3 9 on 4
   3. Given to solve 12 on 1 3 on (4)
4. Demonstrated 6 on 4 8 on 3
   4. Given to solve 4 on 3 6 on (2)
5. Demonstrated 9 on 2 6 on 3
   5. Given to solve 5 on 2 10 on (1)
6. Demonstrated 8 on 1 4 on 2
   6. Given to solve 8 on 1 2 on (4)
7. Demonstrated 3 on 4 6 on 2
   7. Given to solve 3 on 3 9 on (1)
8. Demonstrated 6 on 2 12 on 1
   8. Given to solve 4 on 4 8 on (2)
9. Demonstrated 6 on 1 3 on 2
   9. Given to solve 10 on 2 5 on (4)
10. Demonstrated 12 on 2 6 on 4
    10. Given to solve 2 on 2 4 on (1)
11. Demonstrated 4 on 1 2 on 2
    11. Given to solve 6 on 4 12 on (2)
12. Demonstrated 5 on 4 10 on 2
    12. Given to solve 3 on 2 6 on (1)
13. Demonstrated 8 on 2 4 on 4
    13. Given to solve 12 on 1 6 on (2)
14. Demonstrated 9 on 1 3 on 3
    14. Given to solve 6 on 2 3 on (4)
15. Demonstrated 2 on 4 8 on 1

15. Given to solve 6 on 3 9 on (2)
16. Demonstrated 10 on 1 5 on 2
16. Given to solve 4 on 2 8 on (1)
17. Demonstrated 6 on 2 4 on 3
17. Given to solve 8 on 3 6 on (4)
18. Demonstrated 3 on 4 12 on 1
18. Given to solve 9 on 4 12 on (3)
19. Demonstrated 6 on 1 2 on 3
19. Given to solve 4 on 3 12 on (1)
20. Demonstrated 2 on 2 1 on 4
20. Given to solve 12 on 2 8 on (3)

The numbers in parentheses indicate the answers which were credited as correct. It will be noticed that no problem is duplicated, and that the demonstrations are just the reverse of the problems given for solution, and given in reverse order. The students were never given the correct answers to the problems they had to solve. The only way they had of testing their principle was to apply it on the demonstrated problems to see if their answers agreed with those shown. In preliminary testing we tried the method of showing the correct answers to the problems after the students had written down their answers, and we found a number of cases where the answers had obviously been corrected.

After the 20th problem was given, I said: "Now turn over your sheet and write out the general principle, that is, write out how you did the problems."

The intelligence tests were graded according to Terman's directions. The weight problems were graded either right or wrong and the number of right answers was used as the individual's score. The statement of the principle was rated as either correct or incorrect. If the individual showed he had a practical working principle, full credit was given regardless of the abstractness of the statement. Thus statements such as "If I had an 8 on 3 I would put a 12 on 2 because 8 times 3 equals 12 times 2 or 24 equals 24," or "I multiplied the weight by the hook on one side and on the other to make them equal" or "I multiplied the weight by the hook on one side and divided by the other weight to get the number of the hook on the other side to put it on" were all given credit. Statements such as "I multiplied to get my answer" or "I divided" were not credited. No doubt, some of these had a working system since they received such high scores, but a number of others with low scores undoubtedly had no method well enough in mind to apply it successfully. In the case of a number of subjects from the Engle-

wood High School, where the group was a physics class, the statements were as abstract as those found in a physics book. The wonder is that 9 out of 31 in this class could not write a statement well enough to be credited, although most of these, no doubt, had a working system, as their high scores on the test indicate.

Summary of Related Literature

The literature in this general field is not very extensive. John C. Peterson (3) was the first, to our knowledge, to publish a study involving problems similar to ours, that is, problems involving a general principle which could be applied in their solution. He had a set of beads strung on a wire and required his subjects to draw alternately with him a given number of beads. The object was to have the last draw in reducing the number of beads to zero. The subjects could win if they drew to a multiple of the sum of the greatest and least possible draws. He had 46 subjects in all and gives an analysis of the method of solution, including introspective reports, in detail for one group of 14 of his subjects. His work relates to ours only incidentally where he makes age comparisons. His subjects in the group of 14 were adults with the exception of one 13-year-old high school boy. Only 2 of the 13 adults required fewer trials and problems than the boy whose time record was the lowest of the entire group.

Peterson says: "Subject iii (the boy) appeared to be less conscious of method, somewhat more reliant on trial and error procedure, and quicker in reaction than subjects i and ii; but in no respect did his responses vary so much from those of these two adults as did those of some of the other adult subjects."

In a preliminary experiment an 11-year-old boy in the seventh grade had a record equal to or superior to his father who also took part in the preliminary experiment. His record was also equal to that of the best subject in the group of 14, but the conditions were not quite the same.

These results might indicate that the ability to generalize does not increase with age but, in view of the fact that the comparisons are based on two cases, no confidence can be placed in the findings.

Joseph Peterson (4) tested the ability of 19 subjects in a problem requiring the association of letters and numbers. The correlation of rankings on "fewness of errors" to estimated intelligence was .86

and to marks based on six examinations in general psychology was .96.

In a later study (5) he gave the same type of problem to 81 college students. The correlation with psychology marks was .63±.04. This type of problem does not involve a general principle in the same sense that ours does. Mention is made of his studies merely because he regards the problems as involving rational learning and because of their correlation to intelligence and school marks.

Haught (2) continued Joseph Peterson's studies, using the same type of problem and also a modified form, and added two problems, a "checker puzzle" and the "Tait labyrinth puzzle." He intercorrelated these and also correlated each to Binet-Simon intelligence scores in the case of 74 college students. The intercorrelations ranged from .18 to .44, and the correlations with test intelligence ranged from .26 to .47. His conclusion pertinent for our study is that his rational learning problem and his modified rational learning problem test something not detected by intelligence tests but that the other two problems do not. He fails to give the reliability of any of his tests and we cannot estimate the influence of chance in determining the results.

Waters (7) used problems similar to those of John C. Peterson to study the influence of tuition on ideational learning. One of his groups received demonstrations as a form of guidance. In this case the experimenter drew first to win, giving two such demonstrations for each "set," after which the subjects drew first. This type of guidance was the only one to exert "a detrimental effect both on the subject's ability to master the first problem and also on his ability to deal with the second problem."

All of our groups were given demonstrations since it appeared to be the only way they could learn the principle under our conditions of group testing without giving them the principle directly. One group, however, was given only half as many demonstrations as the others and its mean score was significantly less than that of a comparable group. Waters did not vary the amount of demonstration to see if increasing it increased its efficacy.

Carroll (1) analyzed the errors made by fourth- and fifth-grade students in spelling unfamiliar words and concluded that bright students are able to generalize phonetically better than dull children. However, this type of generalization is not yet well defined since it cannot be stated as an invariable rule which will give a solution to

all the specific problems to which it might be applied and hence it cannot be measured directly.

Correlations

Pearson correlation coefficients were computed, using individual raw scores rather than grouping the data and using a correlation data sheet. All correlations not having a negative sign are positive. The following general formula was used:

$$r_{xy} = \frac{\frac{\Sigma_{xy}}{n} - \left(\frac{\Sigma_x}{n} \cdot \frac{\Sigma_y}{n}\right)}{\sigma_x \sigma_y}$$

Table 2 shows the value of the zero-order coefficients with their standard errors. They are carried to four places not because we consider these places significant but in order to secure closer agreement in our partial correlations dependent upon them.

TABLE 2
Zero-Order Coefficients

r and *P.E.*	Variables
r_{12} .5363±.031	age and intelligence
r_{13} .4587±.034	age and generalizing
r_{23} .4049±.036	intelligence and generalizing
r_{14} .8670±.011	age and grade
r_{24} .7573±.018	intelligence and grade
r_{34} .5584±.030	generalizing and grade

The subscripts of the correlation coefficients are as follows:

1 is age
2 is intelligence test scores
3 is weight problem scores
4 is grade in school

In general, the variable associated with the highest correlations is grade in school and the variable associated with the lowest correlations is weight problem scores.

Reliability

Since the correlations of the other variables with the weight problem scores are lower than we expected, indicating that we are apparently measuring something rather independent of these variables, and especially of age and intelligence test scores, we next

decided to find out if we were measuring anything at all, and hence tested the reliability of our measure. The only method at our disposal for this test was to correlate the odd and even items in the score. We then applied the Spearman-Brown formula to obtain the reliability for the original length of our test. The reliability was determined for each grade group and also for the entire range of data. The results are given in Table 3.

The average reliability coefficient for a single grade group is about .84. The average for the primary schools is .83 and for the secondary schools is .84. There is one instance of especially low reliability, that of the Englewood High School, where it is .50. This group was a physics class in which 22 out of 31 students received perfect scores, and must have equal odd and even scores. This fact is sufficient to explain the low correlation in this case and the coefficient is probably an underestimation of the reliability. If we do not include this low instance, the average reliability coefficient for all schools is .86 and the average for the secondary schools .90.

TABLE 3
RELIABILITY OF WEIGHT SCORES

Grade	r	By the Spearman-Brown Formula
5B	.64	.78
6B	.70	.82
6A	.82	.90
7B	.64	.78
7A	.59	.74
$8B_1$	.86	.92
$8B_2$	.71	.83
8A	.81	.89
9B	.87	.93
9A	.81	.89
10B	.86	.93
10A	.80	.89
11B	.67	.80
11A	.34	.50
12B	.92	.96
All grades	.907	.95

The reliability coefficient for the entire range of grades is .907, which becomes .95 by the Spearman-Brown formula. Although higher reliability would be desirable, we can be safe in assuming that our test is measuring something in which chance plays only a small part in determining the result or score.

Partial Correlations

We next computed the first-order partial coefficients necessary for those second-order partials in which we were interested. The first-order partials, with their standard errors, are given in Table 4.

It will be noticed that three of these coefficients become negative, those in which grade in school is partialed out. Among these are the correlations between weight problems and intelligence, namely, —.0615, and weight problems and age, namely, —.0332. Although these negative findings are too small to be significant, the reductions of the correlations to practically zero are interesting.

TABLE 4
First-Order Partial Coefficients

$r_{13.4}$ equals —.0615±.043	$r_{23.1}$ equals .2119±.041
$r_{12.4}$ equals —.3696±.037	$r_{34.1}$ equals .3619±.037
$r_{23.4}$ equals —.0332±.043	$r_{24.1}$ equals .6952±.022
$r_{13.2}$ equals .3130±.039	$r_{34.2}$ equals .4217±.035
$r_{14.2}$ equals .8361±.013	

Finally, we computed the second-order partial coefficients and their standard errors, shown in Table 4.

TABLE 5
Second-Order Partial Coefficients

$r_{13.42}$ equals —.0794±.043.	$r_{34.12}$ equals .3069±.039
$r_{13.24}$ equals —.0795±.043	$r_{34.21}$ equals .3071±.039
$r_{23.14}$ equals —.0603±.043	$r_{24.13}$ equals .6789
$r_{23.41}$ equals —.0603±.043	$r_{14.23}$ equals .8176

Two solutions are given as a check for the partial coefficients in which the weight problems are one of the unpartialed variables. We are mainly interested in these correlations, although the two coefficients, in which the weight problems are partialed out, are given for comparison.

It would appear from these findings that the variable we are most interested in, namely, the ability to solve problems involving a general principle, is not very closely related to any of the other three variables. We had not expected the correlation of this variable to intelligence test scores to become zero by partialing out the influence of grade in school and age. We had also expected age to be more closely related to this variable than is the case. When the other

variables are held constant, the correlation between grade in school and the weight problems is only .30, not a very high relationship. Reference to Table 7, comparing the standard deviations to the standard errors of estimate shows, in the case of this correlation, that the predictive value is low. The standard deviation of the weight problems for all subjects is 4.93 and the standard error of estimate is 4.69 when the correlation coefficient is .30.

Multiple Correlation and Prediction

In order to obtain the highest predictive value the three other variables had for the weight problems, we determined the multiple correlation coefficients. The results are shown in Table 6.

TABLE 6
Multiple Correlation Coefficients

$r_{3.12}$	equals $.4958 \pm .032$
$r_{3.24}$	equals $.5591 \pm .030$
$r_{3.14}$	equals $.5607 \pm .030$
$r_{3.124}$	equals $.5629 \pm .029$

Again, grade in school is shown to be the important variable in predicting scores on the weight problems. Intelligence adds little or nothing for predictive purposes, since, if we compare the standard errors of estimate to the standard deviation of the weight problems, given in Table 7, we see that the standard error of estimate for the combined age and grade variables is 4.08 in predicting weight problem scores, while adding the variable intelligence reduces this error to only 4.07.

Table 7 gives the value of all the correlations discussed so far, for the purpose of comparing the standard errors of estimate to the standard deviations. For those who are statistically minded, the comparisons may be made directly in terms of $k=\sqrt{1-r^2}$, the coefficient of alienation. We have also listed the value of the standard deviations and of the standard errors of estimate to illustrate how much is gained in predicting one variable when the value of the other is known.

The variable giving the least error for predicting scores in the weight problems is grade in school. Its error is, in fact, lower than that of the best combination of the other two variables, intelligence

TABLE 7
PREDICTIVE VALUE OF TESTS

Variables	r	k	σ of 1st variable in subscript of r	σ estimate 1st variable	σ of 2nd variable in subscript of r	σ estimate 2nd variable
r_{12}	.5363	.8440	23.44	19.78	39.43	33.28
r_{13}	.4587	.8885	23.44	20.83	4.93	4.38
r_{23}	.4049	.9143	39.43	36.05	4.93	4.51
r_{14}	.8670	.4983	23.44	11.68	3.94	1.96
r_{24}	.7573	.6530	39.43	25.75	3.94	2.57
r_{34}	.5584	.8295	4.93	4.09	3.94	3.27
$r_{13.4}$	—.0615	.9981	23.44	23.40	4.93	4.92
$r_{12.4}$	—.3696	.9292	23.44	21.78	39.43	36.64
$r_{23.4}$	—.0332	.9994	39.43	39.41	4.93	4.93
$r_{13.2}$	.3130	.9497	23.44	22.26	4.93	4.68
$r_{14.2}$	.8361	.5486	23.44	12.86	3.94	2.16
$r_{34.2}$	.4217	.9067	4.93	4.47	3.94	3.57
$r_{23.1}$	.2119	.9773	39.43	38.53	4.93	4.82
$r_{34.1}$	.3629	.9318	4.93	4.59	3.94	3.67
$r_{24.1}$	.6952	.7188	39.43	28.34	3.94	2.83
$r_{13.42}$	—.0794	.9968	23.44	23.36	4.93	4.91
$r_{13.24}$	—.0796	.9968	23.44	23.36	4.93	4.91
$r_{23.14}$	—.0603	.9982	39.43	39.36	4.93	4.92
$r_{23.41}$	—.0603	.9982	39.43	39.36	4.93	4.92
$r_{34.12}$	.3069	.9517	4.93	4.69	3.94	3.75
$r_{34.21}$	.3071	.9517	4.93	4.69	3.94	3.75
$r_{24.13}$	.6789	.7342	39.43	28.95	3.94	2.89
$r_{14.23}$	.8176	.5758	23.44	13.50	3.94	2.27
$r_{3.12}$	.4958	.8684	4.93	4.28		
$r_{3.24}$	.5591	.8291	4.93	4.09		
$r_{3.14}$	.5607	.8280	4.93	4.08		
$r_{3.124}$	.5629	.8265	4.93	4.07		

and age. The standard error in predicting weight problem scores from grade in school is 4.09, while the standard error of the multiple correlation involving intelligence and age is 4.28.

We want especially to call attention to the standard error of the multiple coefficient where all three variables are used to predict the weight problem scores. It is 4.07; the standard deviation for the

weight problems is 4.93. In other words, the multiple coefficient of .56 is not especially valuable for predicting scores on the weight problems.

Correcting for Attenuation

Knowing the reliability coefficient, we were able to correct our correlations for attenuation. We assumed that grade and age were measured perfectly reliably. Terman (6) reports a reliability of above .90 for the intelligence test for single grade ranges but does not report for our range. Since the weight problems have a reliability less than this for single grades, we assumed that the intelligence test would have at least the reliability of the weight problems for our range, and probably higher. We assumed a reliability of .95 for the lower limit and perfect reliability for the upper limit and found that these limits did not effect the corrected coefficients very markedly. The coefficients, corrected for attenuation, are given in Table 8.

TABLE 8

Correlations Corrected for Attenuation

r_{13}	equals	.47			
r_{34}	equals	.57			
r_{23}	equals	.41	(where R_{22}	equals	1.00)
r_{23}	equals	.42	(where R_{22}	equals	.95)

Very little is gained by these corrections. In fact, no matter how we manipulate our data statistically, we seem always to arrive at the conclusion that we are measuring something that is not measured to any great extent by our other variables.

Common sense would have us include the ability to derive and apply general principles as a part of the intellectual functions of human beings. We cannot prove that our problems measure this ability, but it seems logical to assume that they measure it rather than any other special ability. It may be that such ability is never general but always a function of the specific situations which involve different general principles. If we assume that we have at least measured one type of these specific situations, it appears, at least, that intelligence tests do not measure it. Apparently, in their attempt to measure "intelligence" they overlook one phase of it which would ordinarily be regarded as important.

Trends

In order to study trends of means, standard deviations, and coefficients of correlation, we have computed these measures separately for our different grade groups. The results are shown in Tables 9 to 13.

Table 9 gives the mean ages in months of our different grade groups, the standard errors of these means, and the standard deviations. The relation between mean age and grade in school is almost linear, with but one inversion at Grade 7A. Table 10 shows the same measures for the intelligence test scores. Here there are four inversions, at Grades 7B, 8A, 9A, and 10B. The same measures are shown for the weight problems in Table 11. Here there are five inversions, at Grades 7B, 7A, 10B, 10A, and 12B. There is a sixth at Grade 8A if we compare it to the Grade $8B_1$ group with a mean score of 13.00. However, the other $8B_2$ group has a mean score of 11.24, and it appears that the mean of 13.00 for Grade $8B_1$ is more likely to be in error than that of the 8A group.

In general, the means of the three variables increase as we progress through the school grades. None of the inversions are greater than three times the standard errors of the differences and may be assumed to be due to chance selection. There is no noticeable trend in the case of the standard deviations, these remaining about the same throughout the grades.

TABLE 9
Trend of Means of Age

Grade	n	Mean age	Standard error	Standard deviation
5B	42	132.1	1.87	12.11
6B	42	147.2	1.74	11.29
6A	43	147.9	1.67	10.98
7B	40	160.8	1.88	11.86
7A	38	158.0	1.81	11.13
$8B_1$	35	161.4	1.61	9.54
$8B_2$	42	164.7	2.31	14.98
8A	41	166.7	1.57	10.05
9B	39	168.9	1.67	10.45
9A	32	179.5	1.83	10.33
10B	31	184.7	2.24	12.50
10A	30	189.8	1.62	8.86
11B	26	202.6	1.86	9.49
11A	31	200.7	2.62	14.60
12B	27	204.9	1.82	9.48
All groups	539	168.3	1.01	23.44

TABLE 10
TREND OF MEANS OF INTELLIGENCE

Grade	*n*	Mean intelligence	Standard error	Standard deviation
5B	42	38.7	2.44	15.83
6B	42	62.9	3.81	24.72
6A	45	64.4	2.89	18.95
7B	40	63.6	3.42	21.66
7A	38	65.0	3.27	20.14
$8B_1$	35	86.8	4.39	25.97
$8B_2$	42	92.0	3.22	20.86
8A	41	88.6	4.36	27.90
9B	39	121.5	4.56	28.47
9A	32	117.6	3.96	22.41
10B	31	108.6	5.60	31.17
10A	30	126.7	5.70	31.21
11B	26	127.9	5.30	27.05
11A	31	130.9	4.92	27.38
12B	27	148.7	4.23	21.99
All groups	539	92.1	1.70	39.43

TABLE 11
TREND AND MEANS OF WEIGHT PROBLEM SCORES

Grade	*n*	Mean wt. problems	Standard error	Standard deviation
5B	42	9.50	.56	3.61
6B	42	9.76	.63	4.06
6A	43	10.88	.67	4.38
7B	40	10.23	.51	3.25
7A	38	9.97	.59	3.65
$8B_1$	35	13.00	.79	4.70
$8B_2$	42	11.24	.58	3.78
8A	41	12.12	.74	4.72
9B	39	14.05	.75	4.69
9A	32	15.72	.66	3.74
10B	31	14.03	.80	4.44
10A	30	15.50	.72	3.97
11B	26	17.92	.43	2.17
11A	31	19.26	.24	1.36
12B	27	17.04	.83	4.33
All groups	539	12.92	.21	4.93

Table 12 shows the correlation coefficients of age and intelligence (r_{12}), age and the weight problems (r_{13}), and intelligence and the weight problems (r_{23}). There standard errors are also given, showing that individually they do not possess much reliability. Taken collectively, however, they indicate certain tendencies. Thus, in the case of age and intelligence, the correlations are negative in 13 out

TABLE 12
TRENDS OF CORRELATION COEFFICIENTS

Grade	Age and intelligence r_{12}	Standard error	Age and wt. problems r_{13}	Standard error	Intelligence and wt. problems r_{23}	Standard error
5B	—.32	.14	—.12	.15	.15	.15
6B	—.52	.11	—.08	.15	.37	.13
6A	—.57	.10	—.25	.14	.53	.11
7B	—.30	.14	.37	.14	.20	.15
7A	—.37	.14	—.03	.16	.38	.14
$8B_1$	—.32	.15	—.04	.17	.26	.16
$8B_2$	—.55	.11	.04	.15	—.04	.15
8A	—.36	.14	—.04	.16	.30	.14
9B	—.51	.12	—.31	.14	.62	.10
9A	—.38	.15	—.003	.18	.04	.18
10B	—.39	.15	—.01	.18	.51	.13
10A	.03	.18	—.06	.18	.38	.16
11B	—.11	.19	—.05	.20	.17	.19
11A	—.29	.16	—.04	.18	.47	.14
12B	.13	.19	.04	.19	.09	.19

of 15 instances. Likewise, there is a negative relationship between age and the weight problems in 12 out of 15 cases. There is a positive relation between intelligence and the weight problems in 14 out of 15 cases.

If we partial out the influence of each of the other variables in their relation to the weight problems, the correlations become positive but low. Table 13 shows these results. The relation between age and weight problems with intelligence partialed out is positive in 12 out of 15 cases, and the relation between intelligence and the weight problems is positive in 14 out of 15 cases when age is partialed out.

It is interesting to note that these relations are positive when this direct method of holding the influence of grade constant is used, while the same relations are slightly negative when grade is partialed out by the indirect partial correlation technique. The discrepancies in the case of age and the weight problems are slight, but appear significant in the case of intelligence and the weight problems. Thus, the partial correlation of intelligence and the weight problems with age and grade partialed out was given at —.0795, or about —.08. The average of the 15 correlations, taken directly from the separate grades, between intelligence and the weight problems with age partialed out is about .31. We are inclined to place greater confidence in the latter since it is based on 15 instances, only one of which is negative.

Comparisons of Those Who Were Given Credit for the Principle to Those Who Were Not Given Credit

A comparison of the average scores made on the weight problems by those who were given credit for stating the principle to those who did not receive credit, is given in Table 14. The average score

TABLE 13
Trends of Partial Coefficients

Grade	$r_{13.2}$	$r_{23.1}$
5B	—.08	.12
6B	.14	.39
6A	.07	.49
7B	.46	.35
7A	.13	.42
$8B_1$	.05	.29
$8B_2$	.02	—.02
8A	.08	.31
9B	.09	.57
9A	.01	.04
10B	.24	.55
10A	—.05	.38
11B	—.03	.17
11A	.11	.48
12B	.03	.09

TABLE 14
Comparison of Means of Those Credited to Those Not Credited with Principle

Grade	Total mean	Mean of those credited	Mean of those not credited
5B	9.50	16.33	8.97
6B	9.76	15.75	9.13
6A	10.88	19.00	9.57
7B	10.23	13.80	9.71
7A	9.97	15.67	9.49
$8B_1$	13.00	17.08	10.87
$8B_2$	11.24	13.00	10.76
8A	12.12	17.50	9.90
9B	14.05	16.75	12.17
9A	15.72	17.65	12.50
10B	14.03	18.64	11.50
10A	15.50	18.07	12.93
11B	17.92	19.00	15.00
11A	19.26	19.68	18.22
12B	17.06	18.89	12.63
All groups	12.92	17.75	10.58
All primary schools	10.80	16.19	9.72
All secondary schools	16.08	18.44	13.02

of those who received credit was greater than that of those who did not receive credit in every case for the 15 separate grade groups. The standard errors were not calculated for these groups since the number of cases in the subgroups is so small as to make this concept useless. The reliability stands on the consistency of the reults.

The mean score of all those who received credit is 17.75, while that of all those who did not receive credit is 10.58. Of those who received credit, the mean score of those in the primary schools is 16.19, and of those in the secondary schools, it is 18.44. Also, of those who did not receive credit, the mean score among the primary-school students is 9.72 and, among the secondary-school students, it is 13.02. But the mean score of all primary-school students is 10.80 and, of all secondary-school students, it is 16.80. The larger increment in the last case than in the two preceding cases is due to the fact that more students received credit for the principle in the secondary schools than in the primary schools, a fact which is brought out in Table 15.

If we study the trends of means for both the credited and non-credited groups, we do not find as marked a differentiation as we get for the total groups. Furthermore, if we examine the inversions in the trend of the total means, namely, Grades 7B, 7A, 10B, 10A, and 12B, and compare, in Table 15, the percentage who received credit in these groups as against the percentage of their preceding groups, we find a decrease in every instance. Also, the high mean score of 13.00 for the $8B_1$ group is accompanied by a high percentage, namely, 34% of those credited with the principle.

Table 15 shows the incidence of those credited with the principle and the trend in the percentage of those credited as we go through the grade groups. It also shows the number of males and females in the groups and the number and percentage of each of these who were credited with the principle. There is an increase in the trend of percentages as we advance through the grades, but it is not very constant. The chance presence or absence of but a few individuals capable of grasping the principle of the problems can alter the value of the percentage markedly.

Sex Difference

Beyond the first two grade groups, the percentage of males credited with the principle is higher than females so credited in all but two instances, and in one of these the percentages are equal (see Table

TABLE 15
ANALYSIS OF THOSE CREDITED WITH THE PRINCIPLE

Grade	Total No.	No. males	No. females	Total No.	%	No. males	% males	No. females	% females
5B	42	24	18	3	7	1	4	2	11
6B	42	17	25	4	10	1	6	3	12
6A	43	23	20	6	14	4	17	2	10
7B	40	18	22	5	12	3	17	2	9
7A	38	18	20	3	8	3	17	0	0
$8B_1$	35	15	20	12	34	7	47	5	25
$8B_2$	42	20	22	9	21	6	30	3	14
8A	41	18	23	12	29	7	39	5	22
9B	39	16	23	16	41	6	38	10	43
9A	32	7	25	20	62	5	71	15	60
10B	31	22	9	11	35	9	41	2	22
10A	30	10	20	15	50	5	50	10	50
11B	26	26	0	19	73	19	73	—	—
11A	31	31	0	22	71	22	71	—	—
12B	27	4	23	19	70	4	100	15	65
All groups	539	269	270	176	33	102	38	74	27

TABLE 16
MEAN SCORES OF BOTH SEXES

Grade	Mean score: males	Mean score: females
5B	9.71	9.22
6B	10.23	9.44
6A	12.08	9.50
7B	10.56	9.95
7A	11.17	8.90
$8B_1$	14.27	12.05
$8B_2$	12.15	10.41
8A	13.44	11.09
9B	13.75	14.26
9A	16.00	15.64
10B	15.05	11.56
10A	16.30	15.10
12B	19.50	16.61

15). In the first two grades the incidence of those who were credited with the principle is so small that it has little weight in determining the scores made by the males and females. In the other cases the sex differences in these percentages correspond to the sex differences in scores on the weight problems.

Two of the groups, Grades 11B and 11A, were all males and cannot be used in studying sex differences. Table 16 shows the mean scores of the two sexes for the 13 remaining groups. Since the number

of individuals in each group is small, standard errors are not used. Greater reliance can be placed on the consistency of the result.

There is a difference in favor of the males in 12 of the 13 cases. If we assume the chances to be even that the males exceed the females, the chances of getting 12 such groups out of a total of 13 would be 13 out of 8192 or about 1 out of 630.

With the data which we have it was possible to test two hypotheses to account for these sex differences.

1. Since the correlations between age and the weight problems are slightly negative in all but three instances for the different grade groups (see Table 12), it might be that the females are slightly older except in these three instances and in the case where their weight score exceeds that of the males. Table 17 shows the differences in mean age expressed in terms of the males. A negative sign indicates that the females were older.

The females are older than the males in three instances, at Grades 7B, 7A, and 10B, in two of which they should be older to meet this hypothesis, namely, at 7A and 10B. The males are older in the other nine instances, although, according to the hypothesis, they should be older only at Grade 9B, where the sex differences favor the females, and in Grades $8B_2$ and 10A where the correlations are positive. Therefore, the facts fit the hypothesis in five out of 13 cases, at Grades 7A, $8B_2$, 9B, 10B, and 10A.

2. The second hypothesis is like the first with the substitution of intelligence for age. Since the correlations between intelligence and the weight problems are positive in all but one instance, at Grade $8B_2$ (see Table 12), if the sex differences are due to differences in intelligence, we should expect the males to have a higher intelligence score in all but this case and the case where the females have a higher weight problem score, at 9B. Table 17 shows that the males have a lower score at Grades 5B, $8B_1$, 9A, and 12B and a higher score at Grades $8B_2$ and 9B. Hence the facts fit this hypothesis in 7 out of 13 cases, at Grades 6B, 6A, 7B, 7A, 8A, 10B, and 10A.

The "age" hypothesis fails to account for sex differences at Grades 5B, 6B, 6A, 7B, $8B_1$, 8A, 9A, and 12B. The "intelligence" hypothesis fails at Grades 5B, $8B_1$, $8B_2$, 9B, 9A, and 12B. Thus, they both fail in four instances, at Grades 5B, $8B_1$, 9A, and 12B, and one tends to counteract the effect of the other at Grades 6B, 6A, 7B, 8A, $8B_2$, and 9A. In other words, the only grades in which both hypotheses

clearly succeed in accounting for the sex differences are 7A, 10B, and 10A; that is, in three cases out of thirteen.

On the other hand, if we examine the percentage of males and females who were credited with the principle (Table 15), we find that a higher percentage of females received credit only at Grades 5B, 6B, and 9B. The percentages are equal at Grade 10A. We have already shown that those who receive credit tend to make higher scores than those who do not receive credit (Table 14). Since the females have a higher mean score at Grade 9B than the males, the sex differences in mean scores coincide with sex differences in percentages credited with the principle in all but two cases, at Grades 5B and 6B. In these two cases the number receiving credit is so small that the chance presence or absence of one individual would markedly alter the percentage. We seem justified in concluding, then, that the sex differences in favor of the males are significant though slight, and cannot be accounted for by differences in intelligence or age.

The Influence of Tuition

In our introduction we mentioned that we had one group which was given only 10 demonstration problems. This group has not been included in any of the foregoing work. It was tested at Parker High School, Grade 10B. Table 18 compares the results of this group to the 10B group which had 20 demonstration problems. The mean age and intelligence scores of Parker were higher than

TABLE 17
Sex Differences in Favor of Males

Grade	Diff. in wt. problem	Age diff.	Intelligence diff.	Diff. in % credited
5B	.49	5.56	— 3.02	— 7
6B	.79	.71	13.19	— 6
6A	2.58	2.64	8.64	7
7B	.61	— 1.81	8.40	8
7A	2.27	— 3.43	9.39	17
$8B_1$	2.22	6.95	— 8.22	22
$8B_2$	1.74	5.89	2.39	16
8A	2.35	5.81	5.74	17
9B	— .51	5.43	.81	— 5
9A	.36	2.79	— 3.91	11
10B	3.49	— 4.44	10.53	19
10A	1.20	1.55	20.70	0
12B	2.89	10.40	—45.48	35

TABLE 18
INFLUENCE OF TUITION

	Mean age	Mean intell.	Mean wt. prob.
Parker 10B (10 demonstrations)	189.26	121.08	12.76±.69
Fenger 10B (20 demonstrations)	184.70	108.70	14.03±.80
Difference			1.27
Standard error of difference			1.05
Ratio or σ value of difference			1.21
Area of curve below 1.21 σ			.11314

those of the normal group, so that we should also expect the mean score on the weight problems to be somewhat higher if the number of demonstrations given had no influence on the score. However, this score is lower by 1.27, whereas the standard error of the difference is 1.05. It is, therefore, 1.21 times as large as the standard error. The area of the normal curve below 1.21 σ is .11314. We may then say that the chances are about 1 out of 9 that the larger mean score of our normal group is due to chance. The only factor that we know to have varied and which we can assign this difference to is the greater amount of demonstrational guidance of the normal group.

SUMMARY AND CONCLUSIONS

1. The ability to solve problems involving a general principle may be measured with considerable reliability, indicating that we are measuring a variable in which chance plays only a small part in determining the score of an individual.

2. There is an interrelation which results in positive coefficients of correlation between the four variables measured in this study, namely, age, intelligence test scores, the ability to solve problems involving a general principle, and grade in school.

3. There is no relation between age and the ability to solve problems involving a general principle when grade in school is held constant, either indirectly by means of the partial correlation technique, or directly by computing the correlations between age and the weight problem scores for each grade.

4. When grade in school is held constant indirectly by means of the partial correlation technique, there is no relation between intelligence scores and the ability to solve problems involving a

general principle. But when grade in school is held constant directly by computing the correlations between intelligence test scores and the weight problem scores for each grade, some positive relation is found between the two variables. However, it is still not high enough to indicate very much in common between these two tests. We therefore seem to be measuring a trait commonly thought of as intellectual but which has been neglected by ordinary intelligence tests.

5. There is some positive relation, but not a great deal, between grade in school and the ability to solve problems involving a general principle when age and intelligence are both partialed out. Grade in school also gives the highest zero order coefficient with the weight problem scores. It appears, then, that schooling is more closely related than intelligence, as the latter is measured by the usual type of test, to an ability which many would regard as a significant intellectual function. Our measure of this function involves a principle which is considered important both scientifically and practically by the physicist, engineer, and educator. The schools seem to give a type of training which is general in its effect upon this specific ability, since only one of our groups (Englewood 11A) had specific training involving this physical principle.

6. None of the correlations are very valuable for predicting scores on the weight problems. The best combination of the other three variables has an alienation coefficient of .8265.

7. There is an increase in mean scores in the weight problems as we progress through the school grades. There is also a correlated increase in the percentage of those who understood the principle. These two variables are not perfectly associated, however, since the mean scores of those who were not credited with the principle also increases as we progress through the school grades.

8. There are slight sex differences favoring the males in the ability to solve problems involving a general principle. These differences cannot be accounted for in terms of age or intelligence differences.

9. When the amount of demonstrational guidance is decreased, there is a decrease in the ability to solve problems involving a general principle.

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UNE ÉTUDE EMPIRIQUE DE LA CAPACITÉ À GÉNÉRALISER

(Résumé)

On a fait subir une série de vingt problèmes employant tous le principe de la loi du levier à 577 élèves de l'école élémentaire des "grades" 5 à 12. Alternativement avec ces problèmes on a fait subir vingt problèmes de démonstration pour le rendre possible pour les sujets de dériver le principe et de l'appliquer aux problèmes à résoudre. On a fait une étude de la tendance de la capacité des élèves des divers "grades" de les résoudre, de la corrélation de cette capacité avec les résultats des tests d'intelligence, l'âge, et le "grade" scolaire, de la croissance dans l'incidence de la capacité de donner la principe, des différences de sexe, et au cas d'un groupe d'un "grade," de l'influence de décroître le quantité de direction démonstrative. On a trouvé que (a) la capacité de résoudre les problèmes employant un principe général peut être mesurée d'une façon assez constante, (b) il y a une intercorrélation qui donne des coéfficients positifs de corrélation entre l'âge, le "grade," les résultats des tests d'intelligence, et la capacité de résoudre ces problèmes, mais ces coéfficients sont réduits, en quelques cas pratiquement à nul, par la technique de corrélation partielle, et aucuns ne possèdent une valeur pour les buts prédictifs, (c) il y a une croissance dans les résultats moyens des problèmes et dans le nombre de sujets qui ont compris le principe avec l'avancement dans le "grade" scolaire, (d) il y a de petites différences de sexe qui favorisent les mâles, (e) une décroissance des démonstrations décroît le résultat moyen des problèmes.

Peterson

EINE EMPIRISCHE UNTERSUCHUNG DER FÄHIGKEIT DES GENERALISIERENS

(Referat)

Es wurden 577 Schülern der fünften bis zwölften Klassen der öffentlichen Schulen [d.h. 9 bis 19-jährigen Schülern] eine Serie von 20 Aufgaben vorgelegt die alle das Prinzip des Hebelgesetzes [law of the lever] in Anspruch nahmen. Es alternierten mit diesen Aufgaben 20 Demonstrations aufgaben die gegeben wurden, um die Vpp. dazu zu befähigen, den Grundsatz zu ermitteln und ihn auf die zu lösenden Aufgaben anzuwenden. Man untersuchte die Richtung der Variationen in der Fähigkeit der Schüler der verschiedenen Klassen die Aufgaben zu lösen, die Korrelation dieser Fähigkeit mit an Intelligenzprüfungen erhaltenen Zahlen und mit Alter und Schulstadium, die Zunahme der Verbreitung der Fähigkeit, den Grundsatz zu formulieren, Geschlechtsunterschiede, und, bei einer der Klassengruppen, die Einwirkung der Verminderung der Quantität der Lenkung durch Beweismaterial. Die Befunde waren folgende: (*a*) die Fähigkeit, Aufgaben zu lösen die einen Grundsatz in Anspruch nehmen lässt sich ziemlich zuverlässig messen. (*b*) Es gibt eine Interkorrelation die positive Koeffiziente verursacht zwischen Alter, Klasse, Zahlen an Intelligenzprüfungen, und Fähigkeit diese Aufgaben zu lösen. Diese Koeffizienten werden aber herabgesetzt, in manchen Fällen fast bis auf Nul, durch das Verfahren der Teilkorrelation (partial correlation) und sie sind in keinem Falle zur Vorhersagung nützlich. (*c*) Es zeigt sich mit Erhöhung der Schulklasse eine Erhöhung der an den Aufgaben erzielten mittleren Zahlen und der Zahl der den Grundsatz verstehenden Versuchspersonen. (*d*) Es zeigen sich geringe Geschlechtsunterschiede zu Günsten der männlichen Vpp. (*e*) Eine Verminderung der Beweisführungen vermindert die an den Aufgaben erhaltenen mittleren Zahlen.

PETERSON

FACTORS INFLUENCING TRANSFER BETWEEN FINGER MAZES*[1]

From the Psychological Laboratories of Stanford University

ROGER G. BARKER

This study shows the influence of three factors upon the extent to which knowledge of a human finger maze of one pattern may be transferred to mazes of differing pattern. These factors are as follows:

1. The amount of transfer attributable to knowledge of an original maze when the subject did not know of the relation existing between the patterns of the original and subsequent mazes. One group of subjects worked under the impression that each subsequent maze was an entirely new learning problem, unrelated to the original maze. These subjects are referred to as the uninstructed subjects.

2. The amount of transfer attributable to knowledge of the original maze when the subject had been informed of the relation existing between the patterns of the original and subsequent mazes. One group of subjects was informed of the relation existing between the pattern of the original maze and each subsequent maze before the latter was learned. These subjects are referred to as the instructed subjects.

3. Variations in the amount of transfer attributable to variations in the complexity of the relationship between the patterns of the original and subsequent mazes.

GENERAL TECHNIQUE

1. *Maze Situations Used.* The maze situations devised to test the influence of the foregoing factors are as follows:

a. Situation 1. Figure 1 and Figure 2 illustrate the temporal[2]

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[1]This investigation was conducted in the Psychological Laboratory at Stanford University under the direction of Professor W. R. Miles, to whom the writer wishes to express his appreciation. The paper has benefited from the kindly criticisms of Professors C. P. Stone, J. E. Coover, and Mr. Quinn McNemar.

[2]This nomenclature follows Hunter's (4) designation of an animal maze involving the same principle.

maze used in this experimental situation; this maze has been previously described by Barker (2). It consists primarily of a grooved pathway cut into heavy battleship linoleoum, which is to be traced by the tip of the right index finger. A blindfold subject places his finger at the point marked *s* (Figure 1, see also the position of the subject's finger in Figure 2) and then moves it forward to the intersection of pathways, *B*. Thence four pathways lead away, but at a given time only one of these will be unobstructed. The subject must find the unobstructed or correct pathway by finger exploration. This path leads back to the start *s* in each case. All pathways except this one are blocked by means of wire pegs (diameter, 1/8"; height, 3/16"), which are projected into or removed from the pathways by depressing and releasing levers that extend beneath the board panel and are hinged at the end of the maze nearest the subject (see Figure 2). The free end of each lever is numbered to correspond with the pathway whose peg it operates, hence the experimenter has only to depress the appropriate levers to comply with any formula for a given maze pattern. If the pattern requires that pathway No. 1 be opened first, the experimenter depresses lever No. 1 and holds it down until the subject has passed beyond the position of the peg-obstruction; then it is released and the spring attachment causes it to return to position and to bring the peg back into place. In a similar way, any other required pathway is cleared. In the case of Situation 1, the paths were opened in the following order: 4, 2, 1, 4, 3, and 1.

One of the advantages of this maze is that the subject may be allowed to inspect the apparatus before he is blindfolded and thus to learn the exact nature of the problem he is to solve.

b. Situation 1a. The same temporal maze described above was used in this experimental situation. To the paths of Situation 1, four more paths were added. The paths were cleared in the order 4, 2, 1, 4, 3, 1, 3, 4, 2, and 1.

c. Situation 2. The spatial maze illustrated in Figure 3 was used. Subjects learned the first six intersections. This maze duplicates in a spatial maze the pattern of the *temporal* maze of Situation 1 so far as the order of the choices is concerned; i.e., the direction from the first intersection of Situation 1 is upper left; it is the same in the case of Situation 2; the direction from the second intersection in the case of Situation 1 is lower right; it is the same for Situation 2, etc.

FIGURE 1
THE TEMPORAL FINGER MAZE USED IN MAZE SITUATIONS 1, 1*a*, 3, 4, AND 5

FIGURE 2
THE TEMPORAL FINGER MAZE IN POSITION FOR MAZE SITUATIONS 1, 1*a*, AND 4
The subject is shown with his finger at starting point *S*

FIGURE 3
THE SPATIAL FINGER MAZE USED IN MAZE SITUATIONS 2 AND 2*a*

d. Situation 2a. The entire ten intersections illustrated in Figure 3 were used. This duplicates in a spatial maze the pattern of Situation 1*a*.

e. Situation 3. The maze Situation 1 was rotated 180° in a horizontal plane to provide this situation. Paths were cleared in the same order as in Situation 1.

f. Situation 4. Obtained by using the temporal maze as it was in Situation 1 (Figure 2) and opening the paths in the following order: 3, 1, 4, 3, 2, and 4.

g. Situation 5. The maze as placed in Situation 1 was rotated 90° counterclockwise and the paths were cleared as follows: 2, 4, 3, 2, 1, and 3.

The reliability of these mazes as determined by correlating the number of errors made on the even-numbered trials with the errors made on the odd-numbered trials, neglecting errors made on the first trial to allow for orientation, is as follows:

Maze situation 1:	$r_{\frac{1}{2}\frac{1}{2}} = .90 \pm .03$
Maze situation 1*a*:	$r_{\frac{1}{2}\frac{1}{2}} = .95 \pm .02$
Maze situation 2:	$r_{\frac{1}{2}\frac{1}{2}} = .93 \pm .03$
Maze situation 2*a*	$r_{\frac{1}{2}\frac{1}{2}} = .96 \pm .02$
Maze situation 3:	$r_{\frac{1}{2}\frac{1}{2}} = .92 \pm .03$
Maze situation 4:	$r_{\frac{1}{2}\frac{1}{2}} = .90 \pm .03$
Maze situation 5:	$r_{\frac{1}{2}\frac{1}{2}} = .89 \pm .02$

It will be noticed that there is a variation in the degree of the difference between the patterns of these mazes. Situation 2, for instance, is equivalent to Situation 1 when the latter is extended spatially. These two maze patterns differ completely in the movements necessary for their execution and for this reason provide the subject with different kinaesthetic and sensory data. They are similar, however, with respect to relational content which is largely a matter of applying concepts as to the direction of exits relative to the longitudinal and lateral axes of the subject ("up left," "toward this shoulder," etc.), direction of exits relative to the horizontal and vertical axes of the maze ("first quadrant," "toward this corner," etc.) and direction of exits relative to the direction of previous exits ("opposite the preceding," etc.). Situations 1*a* and 2*a* bear the same relation to each other as 1 and 2. Situation 3 is equivalent to Situation 1 when the latter is rotated 180° in a horizontal plane. Situa-

tions 1 and 3 differ completely in sensory-motor content also, but the nature of their variance is entirely different from that existing between Situations 1 and 2. The relational content having to do with the direction of exits in relation to the axes of the subject is completely reversed; that having to do with the direction of exits in relation to the axes of the mazes and in respect to the direction of previous exits is identical in the two mazes. Situations 4 and 5 differ from Situation 1 in all respects except that having to do with the direction of exits in respect to the direction of preceding exits; i.e., in each case the direction of the exit from the second intersection is diagonally opposite to that of the exit from the first intersection; the exit from the third intersection is between the first two exits and counterclockwise from the second; the exit from the fourth intersection repeats the exit from the first intersection in each case; etc.

2. *The Experimental Procedure.* Subjects met the experimenter singly, by appointment. All the mazes were learned at a single sitting; the time varied from 30 to 90 minutes. In general, the procedure was as follows:

a. Subjects learned a simple finger maze; this is called the *original* maze. Immediately thereafter they learned other mazes the patterns of which bore a definite relation to the pattern of the original maze; these are called the *subsequent* mazes. These subjects will be referred to as the *critical* subjects.

b. Comparable *control* subjects learned the subsequent mazes without having first learned the original maze.

c. The amount of transfer from the original to the subsequent mazes is measured in terms of the error saving accruing to the critical subjects when their scores are compared with the scores of the control subjects.

Both instructed and uninstructed critical subjects learned the original maze under identical conditions.[3] The uninstructed critical subjects learned each subsequent maze as an entirely new problem; the instructions were the same as for the original maze. In the case of the instructed critical subjects, however, the following was added to the usual instructions: "This maze bears a definite relation to the first maze that you learned." The experimenter then explained fully the nature of this relation. For instance, when Situation 1 was the original maze situation and Situation 3 one of the subsequent

[3]See Barker (2) for exact instructions used.

maze situations, the instructions for Situation 3 were as follows: "We use the same apparatus for this maze that we did for the first maze that you learned; in fact, it will be the identical maze turned backward. You know how to follow the paths when the maze is in this position, don't you? [The experimenter placed the maze with all the pegs in place in position for Situation 1.] Now this time it will be turned like this. [The experimenter rotated the maze 180° in a horizontal plane.] You may be able to adapt your knowledge and trace correctly the paths when the maze is in this changed position; or you may be able to trace some of them correctly and have to re-learn the others; or you may have to learn the whole thing again. In any case, it is your job to become able to follow the paths correctly in as short a time as possible. If you can apply the knowledge you gained on the first maze to the maze as it will be changed this time, do so; it will save you time; but if you cannot, learn it anew as quickly as possible." In each case it was the relation between the original and subsequent maze that was revealed to the subject; in no case was the nature of the change in behavior which the relation necessitated suggested.

The size of the critical groups and the order of learning the mazes follows:

	Population	Original situation	Order of learning subsequent situations
Uninstructed critical groups:			
Group *a*:	18	Situation 1	Situations 2, 3, 4, and 5
Group *b*:	16	Situation 2	Situations 2, 3, 4, and 5
Instructed critical groups:			
Group *a*:	24	Situation 2*a*	Situation 1
Group *b*:	12	Situation 1	Situation 1*a*

In order to determine to what extent knowledge of the original situation was transferred to the subsequent situations, it was necessary to have a reliable estimate of what the score of the critical subjects upon the subsequent maze situations would have been without previous knowledge of the original maze situation. The scores of inexperienced subjects, chosen at random from the same population that the critical subjects were from, were taken as the best estimate

of the scores of the critical subjects. The number of subjects serving as controls for each maze situation is as follows:

42	subjects	learned	Situation	1	without	previous	experience.
15	"	"	"	1*a*	"	"	"
16	"	"	"	2	"	"	"
17	"	"	"	3	"	"	"
18	"	"	"	4	"	"	"
15	"	"	"	5	"	"	"

3. *Subjects' Reports.* The uninstructed critical subjects were asked, *after they had finished the mazes,* if they had recognized in any case the relation between the patterns of the original and subsequent mazes. The data of the few who had were discarded. The instructed critical subjects were carefully questioned regarding the conscious method they used in transferring knowledge of the original maze to the subsequent mazes.

4. *Scoring.* Only error scores are considered here. An error consisted of a cul-de-sac entered. Retracings and re-entries were negligible, owing to the nature of the mazes and the instructions. The criterion of mastery was three errorless performances out of four successive trials.

5. *Subjects.* Subjects were secured from the elementary psychology classes of Stanford University and San Mateo Junior College.

Results

In Table 1 the mean and median error scores, the standard error, coefficient of variability, and range of the error distributions are given for the control and critical groups upon each of the subsequent maze situations. In Table 2 the following data are presented: (*a*) each mean critical error score expressed as a percentage of the control score upon the same situation; (*b*) the difference between the control error score and the critical error score together with probable error of the difference; (*c*) the ratio of this difference to its probable error; (*d*) the difference between the instructed and uninstructed error score together with the probable error of the difference; and (*e*) the ratio of this difference to its probable error.

By referring to the mean and median number of errors recorded in Table 1, it will be seen that both these measures show that the uninstructed critical subjects made fewer errors upon four of the five subsequent maze situations than did the control subjects. Only in Situation 2, however, is the error reduction significant as shown by the data of Table 2 (row three). The mean of Situation 1 indicates

TABLE 1

MEAN AND MEDIAN ERROR SCORES; STANDARD ERRORS, COEFFICIENTS OF VARIABILITY, AND RANGE OF THE ERROR DISTRIBUTIONS FOR THE SUBSEQUENT MAZE SITUATIONS

		N	Situation 1	N	Situation 1*a*	N	Situation 2	N	Situation 3	N	Situation 4	N	Situation 5
Mean Error Score													
Control		42	28.4±2.6	15	52.6±6.0	16	74.7±7.0	17	24.0±3.0	18	27.0±4.0	15	27.0±3.0
Uninstructed Critical	*a*					18	38.0±3.7	17	21.5±2.0	17	17.8±1.5	17	24.0±2.0
	b	16	30.7±3.0										
Instructed Critical	*a*					24	9.2±0.9	23	5.3±0.7	23	17.4±1.7	22	14.0±1.0
	b			12	9.0±0.8								
Median Error Score													
Control		42	25.0	15	41.8	16	72.7	17	20.0	18	21.6	15	22.5
Uninstructed Critical	*a*					18	32.0	17	11.5	17	14.0	17	22.0
	b	16	22.5										
Instructed Critical	*a*					24	4.0	23	1.4	23	14.0	22	9.0
	b			12	3.7								
Standard Error													
Control		42	22.0±1.3	15	33.5±4.0	16	40.5±4.8	17	19.0±2.2	18	24.8±2.8	15	20.8±2.6
Uninstructed Critical	*a*					18	25.0±2.4	17	18.0±1.8	17	9.0±0.9	17	11.5±1.0
	b	16	21.4±2.6										
Instructed Critical	*a*					24	4.0±0.4	23	6.0±0.5	23	14.5±1.0	22	13.0±1.1
	b			12	4.2±0.5								
Coefficient of Variability ($\sigma_{dis.}/M$)													
Control		42	.77±.06	15	.64±.07	16	.53±.06	17	.79±.06	18	.92±.09	15	.77±.06
Uninstructed Critical	*a*					18	.66±.07	17	.83±.09	17	.51±.06	17	.50±.06
	b	16	.70±.08										
Instructed Critical	*a*					24	.53±.04	23	1.10±.10	23	.84±.08	22	.93±.09
	b			12	.46±.06								
Range of Error Scores													
Control		42	5-105	15	15-140	16	19-136	17	2-72	18	2-115	15	5-120
Uninstructed Critical	*a*					18	2-103	17	3-66	17	6-39	17	8-48
	b	16	12-189										
Instructed Critical	*a*					24	0-45	23	0-20	23	0-60	22	0-48
	b			12	0-45								

TABLE 2

COMPARISON OF THE ERROR SCORES OF THE INSTRUCTED AND UNINSTRUCTED CRITICAL SUBJECTS UPON THE SUBSEQUENT MAZE SITUATIONS

	Situation 1	Situation 1*a*	Situation 2	Situation 3	Situation 4	Situation 5
Mean error score expressed as percentage of control score:						
Uninstructed critical subjects	108		50	90	66	89
Instructed critical subjects		17	12	22	64	52
Difference between mean control and critical error score with its probable error:						
Uninstructed critical subjects	2.3±4.2		36.7±7.9	2.5±3.6	9.2±4.3	3.0±3.6
Instructed critical subjects		43.6±6.0	55.5±7.0	18.7±3.1	9.6±4.3	13.0±3.1
Above difference divided by its probable error:						
Uninstructed critical subjects	0.55		4.6	0.7	2.1	0.8
Instructed critical subjects		7.2	7.9	6.0	2.2	4.2
Difference between mean instructed and uninstructed critical error score with its probable error:			28.8±3.8	16.2±2.1	0.4±2.2	10.0±2.2
Above difference divided by its probable error:			7.6	7.7	0.17	4.5

that the uninstructed subjects (group *b*) made more errors than the control subjects; the difference, however, is not significant; the median error score for this situation shows a small error reduction.

Considering the scores of the instructed critical subjects, both the means and the medians recorded in Table 1 show these subjects to have made fewer errors upon all the subsequent maze situations than the control subjects. In the case of Situation 4, the error reduction is only 2.2 times its probable error; in the other four cases the reduction is at least 4.2 times its probable error (Table 2, row three). The data of Table 1 also indicate that the instructed critical subjects made fewer errors on the four comparable subsequent maze situations (Situations 2, 3, 4, and 5) than did the uninstructed critical subjects. Reference to Table 2 (row five) shows that the amount of the error reduction was not significant in the case of Situation 4.

The only significant differences in the variability of the error distributions, as shown by the coefficients of variability, are found in the scores of the uninstructed critical subjects upon Situations 4 and 5 (Table 1). Both of these distributions are significantly less variable than the distributions of the control or the instructed subjects' scores on these same situations.

The influence of the complexity of the relation between the patterns of the original and subsequent maze situations upon the amount of error reduction is shown by the data of Table 2. In the first row of this table the critical scores are expressed as a percentage of the control scores. Considering first the scores of the uninstructed subjects, it is seen that there is no systematic relation between the amount of the error reduction and the complexity of the pattern relations as these were evaluated from a priori considerations at the beginning of the investigation. For instance, in transferring from Situation 2 to Situation 1 there was no error saving; the tendency was toward negative transfer, in fact. When the order was reversed, however, and Situation 1 was the original situation, the positive transfer to Situation 2 is indicated by a reduction of 50% in the control errors. The data of this investigation give no intimation as to the causes of this and other variations in the amount of the transfer found. The indication is, however, that the variations are not related to the a priori complexity of the pattern relationships.

In the case of the instructed subjects, the data of Table 2 reveal that the amount of positive transfer is definitely related to the as-

sumed complexity of the pattern relations. The relations between the original maze situation and subsequent Situations 1*a*, 2, and 3 are simple when compared with the relations between the original maze situation and subsequent Situations 4 and 5, and in the former cases the amount of positive transfer is definitely greater than in the latter cases. In the case of Situation 4, the instructed subjects did not make significantly fewer errors than the uninstructed subjects; it appears that in this case the pattern relationship was of such a nature that knowledge of it was ineffective in increasing positive transfer.

There is always the possibility that the groups of subjects do not properly isolate the factors they are intended to isolate. One possibility is that the superiority of the instructed critical subjects was due to superior learning ability. Data bearing upon this possibility were accumulated as follows: (*a*) the mean score of the instructed critical subjects upon the Thorndike College Aptitude Test was 74.2 ± 2.1; the mean score of the uninstructed subjects was 77.4 ± 2.2; (*b*) both groups learned Situation 1 under the same conditions; the mean error score of the instructed critical subjects upon this maze was 27.3 ± 3.4, and the mean error score of the uninstructed subjects was 28.2 ± 3.0. This evidence indicates that there was no marked difference in the ability of the two critical groups. Another possible discrepancy in the groups as theoretically constituted arises from the fact that the correspondence between instructions and comprehension is far from perfect. Although all the instructed critical subjects were given the same information regarding pattern relations, there were doubtless some who failed to comprehend these relations and who therefore worked under practically the same conditions as the uninstructed critical subjects. It was impossible to identify these subjects. The effect of this error of selection is to make the differences found less than the true differences.

Subjects' Reports

The reports of the instructed critical subjects as to the conscious method used in transferring knowledge of the original situation to the subsequent situations were not complete enough to permit evaluation of the efficiency of the various methods. It is possible, however, to identify with certainty the following conscious procedures:

Conscious Method 1. The visual image of the paths as learned in the original maze situation was changed to coincide with the image of the paths as experienced in the subsequent situation. Such reports

as the following characterized this method: "I just imagined the box was turned around; so I followed the paths as I had before." "Each time I came to the button, I'd see the path I had learned to take; but I would think 'now it's turned around, though'; so then I would turn the map I saw around, and see which way the right path went when it turned, and I'd go that way." "It is like a butterfly with four wings, two large and two small; first time the tail end was toward me, now it is away from me; so now I see it as before, only turned around; I still go around the same wings in the same order."

Conscious Method 2. This method differed from Method 1 only in the fact that the subject was, himself, part of the image and was shifted with the image. "I saw myself sitting at the upper end with the maze before me just as when I learned it the first time. I saw myself doing it from that end, and I followed down here." "I turned myself around with the board and did it as before." There was a tendency with several subjects to turn their shoulders and head as far around in the direction the maze was turned as possible. One subject strained to turn his hand around; he said, "I thought the directions in relation to the corners of my palm; so I was trying to get my hand in the same relation to the maze that it was at first." This subject actually succeeded in changing his position as the others did in imagination.

Conscious Method 3. Knowledge was adapted by a logical process. "I thought how I went on the first one; for instance, if it were up this way (*S moved into one path*), I would say, 'but everything's reversed now; so instead of up, it must be down, and instead of being on this side it must be on the other'; then, after I had followed that path around, I would think how I went next time on the original one, but I wouldn't move until I had translated it to this board." "On the original maze I said, 'first button away from me on this side, second button toward me on that side'; now I say the same words, but I immediately change 'away' to 'toward' and 'this' to 'that.'" "*Right* is *left* now and *up* is *down*."

Conscious Method 4. The subject oriented himself in respect to the approaching path, the first exit, or some other stable element in the situation; he then proceeded relative to this orientation point no matter what the absolute direction might be. "I just hunted around till I found the first path; then I knew the second was diagonally opposite; the third, in between; the fourth, a repetition of the first; the fifth, the one not used yet; the sixth, the same as the third." There were many variations of this method.

Discussion

For purposes of discussion the more significant results may be summarized as follows: Subjects who were informed of the nature of the relations existing between the patterns of the original and subsequent mazes made significantly fewer errors upon three of the four subsequent mazes than did comparable subjects not so informed. There is evidence from the experimental literature which confirms this finding. The following citations do not exhaust those available from the literature on transfer; such a bibliography is beyond the scope of the present intention. These are selected for the wide range of the materials involved. Ruger (7), working with tridimensional puzzles, concludes as follows: "In general, the value of specific habits under a change of conditions depended upon the presence of a general idea which would serve for their control" (p. 19). "The mere presence of a vivid image of a closely related puzzle was not sufficient, however, of itself (to secure transfer). A distinct act of analysis was necessary in addition. . . . The mere presence, in the case of changed conditions, of motor habits appropriate to the new conditions did not necessitate positive transfer. It could coexist with negative transfer. . . . The degree of positive transfer varied directly with the precision of analysis of the similarity of the new case to the old" (p. 87). It is the writer's opinion, after a study of Ruger's whole article, that the mental processes he designates as "a general idea which would serve as control," and "analysis of the similarity of the new case to the old" are the same processes designated by the present writer as "knowledge of the pattern relations." Coxe (3) investigated the relation of Latin study to ability to spell English words derived from Latin. He concluded that, though, as usually taught, Latin is ineffective in increasing such ability, the spelling of words derived from Latin can be improved through the study of Latin. Merely pointing out the similarities in spelling between the Latin words and their derivatives produces considerable gain, but not so much as when rules and principles are used. Scholckow and Judd, as reported by Judd (5), in their study of the ability of fifth- and sixth-grade boys in learning to hit with a dart a target placed under water, found that the boys who understood the principles of refraction did not benefit by this knowledge in the practice series, but when it came to transferring to the target placed at a different depth, they were much superior to boys who did not

know the principles of refraction. Squire, as reported by Bagley (1), found that training in neatness in arithmetic papers was "almost startling in (its) failure to show the slightest improvement in language and spelling papers although the improvement in the arithmetic papers was noticeable from the very first" (p. 208). Ruediger (6), however, reports that training in neatness in one school subject when the training was accompanied by emphasis upon the importance of neatness in dress, business, the home, hospitals, etc., did carry over to other school subjects even when no mention of neatness was made in connection with these other subjects.

The theory of "identical elements" as stated by Thorndike (8) is a useful and widely accepted means of handling a complex problem. The problem of transfer will be considered in terms of this hypothesis; it is as follows: "one ability is improved by the exercise of another only when the neurones whose action the former represents are actually altered in the course of the exercise of the latter" (p. 417). The data of the present investigation and of the studies cited offer no direct evidence as to the truth or falsity of this hypothesis. These data suggest, however, that, if exercise of common neurons be necessary for securing transfer, the effectiveness of this exercise may depend upon independent conditioning factors. For instance, in the case of the present investigation both the instructed and the uninstructed subjects (Group *a* in each case) received the same exercise on the original maze; this should have resulted in identical neural alterations in both groups of subjects. The neural alteration in the case of the instructed subjects resulted in improving ability on the subsequent mazes, but in the case of the uninstructed subjects it did not result in similar improvement upon the same mazes. It is evident that the effects of identical alterations of the same neural mechanisms were conditioned by the presence of an independent factor, i.e., instruction. A better statement in regard to transfer would therefore appear to be as follows: alteration of neural mechanism $x \rightarrow$ unknown factor $y =$ alternation in all the abilities in which neural mechanism x is involved.

In each of the cases reported, the obtaining of the maximal amount of transfer was dependent upon the presence of a factor y which was variously called "knowledge of the relation," "a general idea which would serve as control," "analysis of the similarity," "pointing out of similarities," and "understanding of the principles." In the present investigation factor y was not introduced until after exercise

upon the training situation was finished. This suggests that the mechanism may have been in the nature of an inter-connecting mechanism between the neural set-up secured in the training situation and that required in the situation to which transfer was secured. The reports of the subjects of the present investigation offer a basis for constructing an hypothesis as to the nature of this intercommunicating mechanism.

Let it be assumed that with every conscious experience there is a corresponding physical condition; the situation in regard to positive transfer between the mazes of the present investigation may then be conceived of as follows: In the case of Conscious Method 1, for example, the knowledge of the original maze is in the nature of the physical correlate of a visual image of the paths. There is a similar type of physical correlate corresponding to the percept of the subsequent maze. Recognition of the relation between the patterns of the original and subsequent mazes must also have a physical correlate which, it may be, arises when some sort of an equilibrium is struck between the physical correlate of the image of the original maze and the physical correlate of the percept of the subsequent maze. The process of transformation which the subjects experienced as "turning the paths around in my mind," or "shifting the picture of the paths," must be considered as an actual transformation of the physical correlate of the visual image of the original maze to make it correspond to the physical correlate of the percept of the subsequent maze. This transformation is obvious in the case of Conscious Method 3 where knowledge of the original maze is in the form of a series of remembered words; e.g., "upper left," "lower right," etc. Knowledge of the relation between the patterns of the original and subsequent mazes reveals the relation between this series of remembered words and that series which describes the percept of the subsequent maze. When this relation is known the series of known words may be immediately transformed into the series adequate for the subsequent maze. For example, in the case of a complete reversal (Situation 3) "upper left" becomes "lower right," "lower left" becomes "upper right," "upper right" becomes "lower left," etc.

Only in the case of Conscious Method 4 is there identity between the knowledge of the original maze and the percept of the subsequent maze. With this method, the second move is always opposite the first, the third "in between," the fourth a repetition of the first, etc. In all the other methods either a particular visual image trace

has to be changed to coincide with a new image, or particular word traces have to be transformed into suitable, particular words. This means that the identity between the mazes has to be created by the subject; i.e., the original psychological situation (image trace or word trace) is actually changed to correspond to the new psychological situation.

Neither the data of the present investigation nor of those cited give any indication as to the constancy of factor *y* in transfer situations. Educational implications are largely dependent upon the constancy with which this factor operates. One educational implication seems certain, however: the tranfer effects of practice may be negated by the absence of this necessary conditioning factor.

Summary

This study shows the influence of three factors upon the extent to which knowledge of a human finger maze of one pattern (the original maze) may be transferred to mazes of differing pattern (the subsequent mazes). The influence of these factors has been evaluated as follows:

1. The amount of positive transfer attributable to knowledge of the original maze when the subject did not know of the relation (e.g., reversal, spatial extension) existing between the patterns of the original and subsequent mazes was small. On only one of the five subsequent mazes was the amount of transfer statistically significant. It ranged from the amount of positive transfer indicated by a saving of 50% of inexperienced control subjects' errors to the negative transfer indicated by an increase of 8% in the control subjects' errors; only in the former case was the saving statistically significant.

2. The amount of transfer attributable to knowledge of the original maze when the subject had been informed of the nature of the relation existing between the patterns of the original and subsequent mazes was statistically significant on four of the five subsequent mazes. It ranged from the positive transfer indicated by a saving of 88% of the control subjects' errors to that indicated by a saving of 36% of the control errors; in the latter case the saving was not statistically significant.

The subjects who knew of the pattern relations made significantly fewer errors upon three of the four comparable subsequent mazes than the subjects who did not know of the pattern relations.

3. There was no systematic relation between the amount of trans-

fer and the complexity of the relations between the patterns of the original and subsequent mazes, as these were evaluated from a priori considerations at the beginning of the investigation, in the case of the subjects who did not know of the existence of these relations. In the case of subjects who were informed of the existence of these relations there was a rough negative correlation between the complexity of the pattern relation and the amount of transfer.

Subjects' reports of conscious experience while transferring were recorded.

The results are interpreted as indicating that the transfer effects of training on the original maze were conditioned by the operation of an independent factor, i.e., knowledge of the pattern relations.

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LES FACTEURS INFLUENÇANT LE TRANSFERT ENTRE LES LABYRINTHES À DOIGT

(Résumé)

Dans ces expériences on a employé des labyrinthes à doigt temporels et des labyrinthes spatiaux. On a employé sept différents labyrinthes ou situations de labyrinthe. La constance de ces labyrinthes déterminée au moyen de corréler les erreurs faites dans les épreuves paires avec les erreurs faites dans les épreuves impaires, a varié de 0,89±0,02 à 0,96±0,02. Le but de l'étude est de montrer l'influence d'une forme de labyrinthe (le premier labyrinthe) sur l'apprentissage d'une autre forme (le labyrinthe subséquent) qu'on a varié de plusieurs manières. On a instruit quelques groupes de sujets sur cette variation et sa nature temporelle ou spatiale; on n'a pas instruit les autres groupes. Quand le sujet n'a pas su qu'il existe une relation entre les formes du premier labyrinthe et les subséquents, le transfert a été petit. Il a fait une différence statistiquement signifiante seulement dans un des cinq labyrinthes subséquents. Il a varié d'une décroissance de

50% des erreurs du sujet de contrôle sans expérience au transfert négatif indiqué par une croissance de 8%. Quand on a instruit les sujets de la nature de la relation existant entre les formes du premier labyrinthe et les subséquents il s'est trouvé des différences de transfert statistiquement signifiantes dans quatre sur cinq labyrinthes subséquents, lesquelles ont varié d'une décroissance positive de 88% à 36%. Il n'y a pas eu de relation systématique entre la quantité de transfert et la complexité des relations entre les formes, comme ces complexités ont été évaluées des considérations *a priori,* chez les sujets qui ne savaient pas l'existence des relations. Quand les sujets ont su les relations il y a eu une corrélation négative approximative entre la complexité des changements de forme et la quantité de transfert. On conclut que les résultats indiquent que les effets de transfert de l'entraînement sur le premier labyrinthe sont définiment conditionnés par l'opération du facteur indépendant, la connaissance des relations des formes.

BARKER

EINWIRKUNGEN AUF ÜBERTRAGUNGEN VON EINEM FINGER-LABYRINTH (TEMPORAL MAZE) AUF ANDERE

(Referat)

Diese Versuche wurden mit temporalen Fingerlabyrinthen (temporal finger mazes) und mit Raumlabyrinthen (spatial mazes) ausgeführt. Es wurden sieben verschiedene Labyrinthe gebraucht. Die Zuverlässigkeit dieser Labyrinthe wurde dadurch bestimmt, dass man die Korrelation berechnete zwischen den in den mit geraden Zahlen und den in den mit ungeraden Zahlen nummerierten Versuchen gemachten Fehler. Lässt man den ersten Versuch ausser Acht, so Schwankt diese Zuverlässigkeit zwischen .89±.02 und .96±.02. Die Untersuchung versucht, die Einwirkung einer Labyrinthgestaltung (maze pattern) (das ursprüngliche Labyrinth) auf das Erlernen einer anderen Gestaltung (das nachfolgende Labyrinth), die auf verschiedene Weisen variirt wurde, darzustellen. Einigen Gruppen von Versuchspersonen gab man über diese Variation Bescheid; anderen Gruppen gab man nicht darüber Bescheid. Wenn die Vp. nicht wusste, dass zwischen den Gestaltungen des ursprünglichen und des nachfolgenden Labyrinthes ein Zusammenhang bestand, so war die Uebertragung gering. An nur einem der fünf Labyrinthe zeigte sie sich als statistisch bedeutsamer Unterscheid. Dieser erstreckte sich von einem 50-prozentigen Ersparniss an Fehlern bei den ungeübten Kontrollversuchspersonen einerseits bis zur negativen Uebertragung anderseits, die durch eine Zunahme von 8 prozent angedeutet wurde. Wenn den Versuchspersonen über das Wesen der bestehenden Beziehung zwischen den Getaltungen des ursprünglichn und des nachfolgenden Labyrinthes Bericht erstattet worden war, zeigten sich bei vier aus fünf nachfolgenden Labyrinthen Uebertragungsunterschiede die zwischen einem 88-prozentigen und einem 36-prozentigen Ersparniss schwankten.

Es bestand kein standhafter Zusammenhang zwischen dem Betrag an Uebertragung (amount of transfer) und der Verwicklung (complexity) der Zusammenhänge zwischen den Gestaltungen—so wie diese Verwicklungen an Erwägungen a priori abgeschätzt wurden—bei jenen Versuchspersonen die von dem Bestehen der Zusammenhänge nichts wussten. Wussten die Vpp. von den Beziehungen, so fand man eine approximative negative Korrelation zwischen der Kompliziertheit der Aenderungen der Gestaltungen und dem Betrag an Uebertragung. Die Befunde werden gedeutet als Andeutung, dass die Einwirkungen der Uebertragung der Einübung an dem ursprünglichen Labyrinth bestimmt durch die Wirkung des unabhängigen Faktors, des Wissens über die Beziehungen zwischen den Gestaltungen, bedingt werden.

BARKER

NOTES ON THE HORIZON ILLUSION: II*

From the Psychological Laboratories of Stanford University

FRANK ANGELL

In some preceding notes on the "horizon illusion" (2) the writer argued that Zoth's (40) theory of convergence strains in raising the eyes as causing the lessening in the apparent size of heavenly bodies at the zenith was not tenable. In the first place, convergence can no longer be regarded as immediately determinative of distance and, through this, of apparent size; variations in size do not march with variations in convergence; size illusion is greater for far objects with weak convergence than for near objects with strong convergence (35). Further, the figures given to support the theory were both ambiguous and insufficient—ambiguous because in some cases no difference was shown between horizontal and uplifted gaze and insufficient because the differences when shown were too small to account for the size of the illusion. Finally, several skilled and painstaking observers failed to find any decrease in the size of constellations with uplifted gaze.

It was also set forth that in some, perhaps many cases, the illusion had nothing to do with the horizon or horizon distances as such; they were instances of the projection of the sun or moon to or towards known bodies at known or supposed distances. These cases probably include many of the maximal effects; in the writer's experience they include most of the maximal effects. The *Einheitstrieb* is no trustworthy impulse towards explaining a phenomenon as varied and as varying in its aspects as the "horizon illusion." The name itself implies projection to the horizon and so raises the question of the form of the firmament and, while this is a problem by itself, historically, it has been treated as a part of the size-illusion phenomenon and so at least must find mention here.

So far as records go, at all times and in all places the heavens have been held to be a dome—flattened at the top or not as the case may be. That the dome for some was "real," i.e., material, appears from the cosmogony of Aniximander and Empedocles who held that

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the firmament was a crystal dome with the stars fastened to the concave side, the whole structure revolving around the surface of the earth as an observatory dome revolves around the platform of the telescope.

For "such creatures as we are and such a world as the present one" the firmament commonly appears as a flattened dome; as mid-distance between horizon and zenith a point is usually selected considerably less than 45° from the horizon. To Robert Smith, of Cambridge, "Professor of Astronomy and Experimental Philosophy and Master of Mechanics to His Majesty" (George III), the heavens appeared flattened as a matter of observation and must needs so appear as a matter of deduction. Imagine a wall, he argues, indefinitely long, erected on a plain of indefinite extent and viewed at right angles to the line of vision. Five thousand times the height of an observer above the ground, or about 5 miles, is the limit of visible distance; all distances beyond this seem equal. Consequently, the wall to an observer 5 miles or more away would appear circular (36, p. 63).[1] If the plane with the wall upon it were revolved like the lid of a box till the wall were overhead, it would appear concave. The horizontal plane is a visible surface suggesting like distances quite around the eye, but in the vertical plane "there is nothing that affects the sense with an idea of its parts"; consequently, "the apparent distance of the higher parts will be gradually diminished." It is to be noted that this is an explanation of what is actually seen as well as a theory of what must be seen.

But logical necessity is no infallible guide in psychology. Wilhelm von Zehender's observations on the apparent shape of the firmament give a very different impression from that produced on Smith. "Only the blue of heaven" he says, "is perceptible in a cloudless sky. Gaze where I may, I perceive not the slightest trace of a vault effect." With small, fleecy clouds in the air, however, he finds the vault effect *"ganz frappant."* At night he sees the stars, not as fastened to the inner surface of a dome, but as if they were a motionless swarm of gnats (39).

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[1]See also Stratton (37), thesis—vault of heavens, a perspective effect; Jaensch (19, pp. 173-200), effect due to a tendency to localize visual impressions perpendicular to line of vision—to an "orthogone Lokalisationstendenz"; Filehne (11, pp. 279-308). This circular effect is noticeable on great planes like the ocean and the prairies. It is particularly marked on broad marshes with scanty growth and low trees. When the atmospheric perspective is strong so that the outlines of the trees are softened, they seem to be accurately arranged in a vast circle on the horizon.

This difference was represented in the Stanford laboratory. Dr. Coover noted the form of the sky as it appeared to him lying on his back (4 A.M., June 4): "Somewhat unprepared for the apparent flattening, I sought to bisect the line to determine the angle," i.e., the angle formed by the lines drawn from his eye to the point of intersection and the horizon. This angle was about 20°. Other observations gave like results. But Miss G. M. Trace, Instructor of Psychology, reported that in daytime "land and sky do not seem to meet at the horizon any more than a tree that is close by would touch the horizon. Sky is just roominess which is indefinite. At the horizon the sky seems to go right over and does not stop. At night the sky has the same effect of mere roominess and the stars seem to float in it at varying depths from the earth." In this case, as with von Zehender, it is fair to guess that empathic factors are in play, probably induced by steady gazing.

To Eugene Reimann (34)[2] the heavens appeared as a flattened dome with midpoint between zenith and horizon, some 23° from the latter. *Per contra,* at von Zehender's suggestion, Dozent von Sicherer, working in concert with 13 helpers, reported 42° from the horizon as the average mid-distance for all observers, the sky being cloudless (39, pp. 236-237). Von Sicherer himself found 41.1° for a clear day and 47° for a starry night. Now, what was estimated, measured, or gauged as between Smith and Reimann, on the one hand, and von Sicherer and von Zehender, on the other? Surely not the same thing: a difference of about 100% with skilful observers in gauging the mid-distance between horizon and zenith is out of the question. Von Zehender (39, p. 180) says that no *suggestionsverdächtige Bermerkung* was made to his assistants, but he also says they were cautioned to mark the zenithal point with especial care, which may have suggested the bisection of the angle instead of the arc. If for von Zehender the heavens were simply amorphous blueness, it is hard to see what he could have bisected except the arc. He mentions but does not describe an "instrument" which may have suggested angular measurement. Reimann mentions and also condemns an instrument which induced bisection of the angle instead of the arc. Von Zehender gives the ages of his observers—15 to 69—which is not important, but makes no mention of their preconceived opinions of the shape of the heavens or of their manner of viewing

[2]Reimann's article gives a helpful history of the illusion.

them, which is most important. In fact, no attention was given to the attitude—external or internal—of the observers in these investigations, which is hardly surprising in view of their training as physicists or physiologists. Von Helmholtz (13) thought that the "vague, indefinite and changeable idea" we have of the flattened dome of the heavens is derived from their aspect when clouded, while Filehne (11), sweeping his gaze from the zenith to a point on the horizon, saw the heavens as a flat ceiling converging towards the plane of the earth. In fact, in most of the investigations on the horizon illusion one gets scanty or no information on the way the observers viewed the heavens; as in the early days of the reaction experiments, the reagent, having "assumed a comfortable position," was left to his own devices as regards attitude and manipulation of the key, so the observers of the horizon illusion seem to have been left free to glance or stare, to view broadly with or without motions of the body and head as often or for as long a time as they saw fit. But, naturally, spatial qualities will differ with different ways of gazing; the metamorphic outcome of staring at well-known objects has found classic expression in James's description of the effects of "fixing" a familiar word. Now, whether one "fixes" the heavens or views them with a roving gaze makes literally a world-wide difference. Filehne (11, p. 280) notes that when he gazes steadily at a point in the heavens near the horizon he gets the effect of a perpendicular wall, and this holds true for most observers. Likewise, a segment of the horizon line at sea seems perpendicular to the line of vision when viewed steadily, but with roving gaze it becomes a curve. Further, according to the play of attention, we may get surface or depth effects as is the case with Hering's (12, p. 573) dark box and of Katz's (24, p. 8) surface and superficial colors.[3] We have, accordingly, entering into the judgments of the shape of the firmament and of the size and distance of the heavenly bodies, a large number of factors—the factor of prepossession of which Reimann bitterly complains, the influences coming from the lay of the land and the state of the atmosphere—often unsuspected and hardly discernible even when known, the different effects arising from different ways of viewing the heavens, and running through all the play of affective and assimilative factors depending on individual experience. Altogether, here are enough

[3]Note also the different effects of fixed and wandering attention in Jaensch and Schönheim's (21, p. 19) report.

factors to account not only for diversity of theory but substantially enough to make of each case of the illusion a unique experience.

For the general edification of his students, the writer collected each year from a large class in general psychology, opinions (they could hardly be termed estimates) on the size of the rising sun or moon. These opinions gave values running all the way from a 25-cent piece, through a series of household utensils, up to a cartwheel. These judgments were rough enough, but while rough, they were not indefinite; the student reporting the dinner-plate size was usually positive that the rising moon was larger than a 25-cent piece and smaller than a cartwheel. The estimates of the scientifically elect vary as broadly; for Kepler the rising sun seemed *"ingens et gigas"*; Huygens found it twice as large at the horizon as at the zenith; for Witasek it was about as large as a medium sized cartwheel; and Molyneux thought the rising moon ten times larger than at the zenith.[4] *Per contra,* in local scientific society one member remarked that he had never noticed any marked difference between the sizes of the horizon and meridional moons, whilst another, mathematically trained, was intolerant of the question and said "it had no meaning." Bourdon (4, p. 430) notes a small number of savants, particularly mathematicians, for whom an inquiry into the apparent size of distance of heavenly bodies seemed senseless.

But the practical as well as mathematical Robert Smith applied his remarkable ingenuity in the measurement of illusive and elusive things to the measurement of the horizon illusion. Selecting some object in the sky—sun, moon, star, or cloud—which appeared to him half way between horizon and zenith and then measuring the angle made by the line drawn from the eye to the object with the one drawn to the horizon, he constructed the circle of which the divided arc was a segment. The angle of the circle subtended by the arc was found to be 23°, which gave a ratio of 3 for distances to horizon and zenith, respectively. Assuming that, in each case, we project the disk of the sun or moon to the vault of heaven and that apparent size is governed by apparent distance, it follows that objects at the horizon appear about 3 times as large as at the zenith. This finding he strengthened by equating distances between pairs of stars near the horizon with pairs on high and measuring the angle, i.e.,

[4]Similar variations may be seen in the size of the moon as it appears in paintings of twilight landscapes.

their astronomical distances. The measurements also gave the approximate ratio of 3 to 1 (36, pp. 64-65).[5] Further confirmation Smith found in the apparent elongation or the lower limbs of coronae, tails of comets, and rainbows.[6]

Reimann, elaborating on Smith's method, compared superior and inferior distances in the heavens by bisecting arcs of 20, 30, 40, and 50 degrees measured from the horizon under various conditions of cloudiness (9). Applying the formula for reducing his results to the bisection of an arc of 90° Reimann found a narrow range of values around 22° as the apparent midpoint between horizon and zenith.

Now, both Smith and Reimann saw celestial bodies projected to the concave surface of the firmament: "We judge all objects in the heavens to be in this very surface," says Smith, and in this "we" he probably included everyone with normal vision. At any rate, he says nothing to show that he surmised anyone saw or could see heavenly objects as lying elsewhere than in the "very surface" of the firmament, *De facto* this seems to be the opinion of most persons—a probable outcome of tradition, casual observation, and current allusions in literature to the "star-studded sky" or the "star-sewn vault of heaven." When, however, one comes to actual observation of the moon viewed rather as a celestial than as a terrestrial object, and the questions are asked, "How far away does the moon seem to be? Does it seem to be in the firmament?" one gets answers that differ considerably from the traditional impression. Of course, one runs the risk in such an inquiry of bringing about that troubled state of mind of which Bourdon complains—the observers do not give themselves up *"a l'impression naturelle que produisent sur eux ces objets."* The only check—a rough one—is readiness of reply. Deliberation, involving reflection on relations of size and distance are here obviously out of place. Now a typical answer of the "ready" variety was to the effect that the moon seemed about a foot across and a thousand feet away, and it was given with as much readiness as a judgment on the size and distance of a finial ball on the top of

[5]The diagram and explanation of the illusion given by Wundt in the fourth and following editions of the *Physiological Psychology* are derived from this section of Smith's *System.*

[6]The ends of a complete rainbow in the north (January 17, 12M.) which seemed to touch the plane of the earth were estimated by the writer as from 5 to 10 times the breadth of the middle—the range of estimation resulting from the evanescence of the ends.

a high flagpole. That such judgments betray the presence of "favorite numbers" and that they may be given by persons whose estimate of a foot, let alone of a thousand feet, is more or less inaccurate, are matters of no particular significance in the *premises;* significant are the facts that the moon seemed to be at some distance and this distance lay far inside the firmament.

Now, while the shape of the firmament is a problem by itself, the matter of the apparent distance of the zenith is a problem complicated with that of the apparent distance of the celestial bodies and is determined by factors common to both. Bourdon collected estimates of the height of the nocturnal zenith from "naïve" observers; as the nights were clear, it is probable that the presence of the stars played a part in the estimates. Seated on the ground near the wall of a house, with the edge of the roof some 6 meters above, one observer marked on the ground a distance of between 30 and 40 meters as the height of the zenith. Another, under like conditions, indicated twice the height of the edge of the roof (5, p. 403). Obviously, in these cases, the roof was the point *de repère.* "Estimate" here means, of course, an attempt to express a given impression of distance in metric terms. Noteworthy, however, is the influence on apparent vertical distance of nearby narrow structures of great height like wireless towers. The sky above the tower seems raised into a sort of cupola or cap, pushing up into the vault of the sky (38).[7] Of the horizontal *"profondeur apparente,"* Plateau (37) endeavored to make a direct measurement. Projecting an after-image of the moon on a wall, or, rather, getting his son—a youth of *"sagacité d'observations"*—to do it, Plateau found 51 meters as the distance where the projected image and the disk of the moon seemed equal. But this is obviously a case of projection against a known or familiar object and whatever the size of such an object as *"Sehding"*—a variable magnitude not only for different persons but for the same person at different times—that size would determine the size of the after-image.

As noted before, von Helmholtz's projection to the horizon through clear glass of the reflected image of the moon on high was confused partly by the dimness of the horizon and partly by the strong tendency to make the reflecting surface of the glass the plane of projection. By chance, an experiment, *au naturel,* arranged itself

[7]The lift above the local 600-foot tower is obvious to the writer but less clearly defined than seems to have been the case with Fraulein Stücklein's observations.

for the writer in a slowly moving railway car. The full moon rising over San Francisco Bay shone through a large window on the east side of the car and its image was reflected from the window on the west to the eyes of the writer, who, having gazed through the window for about an hour had ceased to be aware of a reflecting surface between himself and the landscape quite as much as if the window had been open. Under these circumstances the image of the moon expanded and contracted rapidly and strikingly according as it was projected towards backgrounds formed by nearby fences, buildings, and by more distant hills. The image was not projected against hills, buildings, etc., in the way an after-image is projected against a screen, i.e., with surfaces apparently coinciding, but the landscape rather appeared as a background outside the focus of attention and indirectly viewed, the apparent magnitudes of objects in the background being roughly determinative of the size of the illusion. There was one noticeable exception to this condition; on one stretch of the road, open fields fell away to the beach with the ocean beyond. But the image in this setting of the far distant ocean horizon was decidedly smaller than when projected towards any part of the landscape; it was like the effect that one gets in projecting an after-image of the moon into a cloudless sky; the image was much smaller than the disk of the moon and for the same reason there were no known objects acting as points *de repère* for the formation of fairly definite space "constructs"; it is to be observed that this formation of space "constructs"—to be explained later—while based on the presence of more or less familiar objects, does not mean "comparison" with them.

During a season of forest fires in Oregon when the haze was sufficient to give the setting sun a deep red-orange tint, but not enough to obscure the blue of the upper sky, the writer employed himself in the late afternoons in projecting after-images of the sun towards different regions of the landscape. A long railway trestle some 400 feet distant from the observer's standpoint served as *point d'appui* for relative distances of projection. Projected side of the sun's disk the after-image appeared of like size with the sun; projected *ins Blaue hinein*—say 25 or 30 degrees above the horizon—it was much smaller. Against the trestle, the image was smaller than when located side of the sun's disk but larger than when viewed on high. "Projected against" anything, of course, means "projected into the setting"; the trestle did not serve as a screen. When the image was brought down to the level of the observer it varied in

size but was always smaller than the trestle image and located between the observer and the trestle, at times but few feet distant. Projected to the horizon when the disk of the sun just touched the horizon line it took on the maximum effect of the illusion, i.e., "wash tub" size.

While, of course, it is the entire setting of an object that enters into the impression of its size or distance, it often happens that a single thing, physically speaking, is chief agent in the effect. Moreover, it is not necessary that such "thing" should be especially noticed. Jaensch (18) found translocation effects in haploscopic experiments from changes in backgrounds unnoticed by his observers. Likewise, when projecting after-images of the setting sun to different points of a range of mountains bounding the horizon, the writer was surprised to get a small image in the middle distance, seemingly without reason for its particular local existence. Investigation disclosed a telephone wire stretched across the field of vision unnoted until looked for and even then hardly discernible through the smoked glass used in the observation. Moreover, impressions of the size and distance of an object may be governed by a very abstract knowledge of its situation. "Abstract" here means an absence of perceptual and the barely noticeable presence of ideative elements in a cognition, e.g., the *"données"* one may have for judging the sizes of objects in a dark room. Thus, among H. Gräbke's (12, p. 296) observers set to judge the size and distance of bright lines exposed in darkness, the closest values were given by those who were casually acquainted with the dimensions of the dark room. Noticeable were the findings of those wholly ignorant of the dimensions of the laboratory; they found the lines smaller and nearer than those who were acquainted, if only casually, with sizes of the laboratory rooms.

But it does not follow that one can govern the apparent size and distances of such objects, especially after-images, by means of spatial imagery. Hering (14, p. 138) concedes that one may control the size and distance of after-images in the field of the closed eyes but confesses that he is unable to do it. Bourdon (5, p. 344) declares he accomplished the effect by a sort of flank attack; he projected the images on a cardboard held at arms length, closed his eyes, manipulated the cardboard as before and so aroused the corresponding after-images. I was unable to change the size of images at will with eyes closed, though they seemed to vary spontaneously from a quarter to a full inch. The vague spatial or verbal imagery detected

(word "distance") seemed hardly a sufficient cause for the change.

Broadly and perhaps vaguely speaking, the rule seems to hold that sizes of objects, and usual distances, diminish with the decrease in spatial components in the environment as in viewing a landscape through a narrow tube; if the tube is split in two and the landscape viewed along the lower half, an almost equal reduction takes place, but viewed along the upper the decrease is small. Viewed with head vertically, reversed objects appear smaller but colors have more vividness (*Eindringlichkeit*)—both phenomena arising from the inhibition of the assimilative processes that, under ordinary circumstances, go to make up our ideas of the workaday world.[8] The spatial situation, as broadly known but not definitely marked, may determine ideas of distance as with Gräbke's observers noted above (p. 141). Entoptic phenomena—phosphenes, after-images, retinal gray —with their minimum of spatial data are usually located between the face and arm's length. For Fechner (10) the dark field in which after-images appeared was of limited extent without depth, and placed immediately before the eyes. To Hering (16) retinal gray appeared as a cloud, close to the face, whilst Katz's (24) observers located it some 30 cm. from the eyes. Jaensch and Reich (20) saw eidetic imagery in the field of the retinal gray but were uncertain as to the distance of the latter (22, p. 233). They found, however, 100 cm. as a neutral region for the localization of the eidetic image under voluntary attention; beyond this the image seemed to move towards the observer; inside the distance, the image moved away. One of Bruno Petermann's (32, p. 376) observers, in complete darkness, had a visual idea of space as a hollow structure with a depth "within reach" (*greifbar*). This careful investigation shows the influence of purely psychical factors in producing not merely spatial structures, but structures which are spatial paradoxes. For this observer, then, space ended with the depth of the hollow structure. An illuminated disk being set in the field of vision, the observer notes that "suddenly there appeared a new thing which had no position in the space and which really was not there at all." Angell and Root (3, p. 262) equated negative after-images in the field of the closed eyes with the same images projected on cardboard. The middle values for 3 observers of a 22 cm. square of paper, 71 cm. distant, were squares of from 2.5 cm. to 3 cm. on the edge and from 5.7 cm. to 7.6 cm.

[8]Kaila gives a vivid description of this effect (22, p. 233).

distant. The common rule (Emmert's Law) of the proportionality of size and distance holds true for "laboratory distances" only. But even this limited proportionality C. Mayerhausen (29, p. 23) thought he had refuted by what he considered the "crucial experiment" of sliding a sheet of paper between the observer's eye and the screen on which the after-image was projected. He "found" no change in the size of the image. But this was because screen and paper formed a common surface; had he pushed the paper up to the vertical median plane between the eyes, he would have 2 images, one for eac heye, proportional in size to the distances of projection.

The oft-cited statements of the patients of Cheselden and Nunelly that, in the early stages of their newly acquired vision, objects seemed to touch their eyes were interpreted by Mill (30) as being the outcome of analogy with tactual perception. "Perception of an object and contact with it were no doubt indissolubly associated." To this Jaensch (19, p. 238) objects that Franz's patient and Kaspar Hauser saw objects as near by and not as touching them. As the records of these observations are scanty, anyone concerning himself with them is entitled to his guess. The writer's guess is that the patients, "young gentlemen" or otherwise, after repeated and painful operations, after durance in the darkness of rooms and bandages, must have felt that the confusing, meaningless, and probably disagreeable experiences of their earliest light impressions were in the eye itself. The term "depth" is fairly innocent of confusing issues in discussions of space-perception partly because depth has often been regarded as a sensation, correlative with "sensations" of breadth and height, and partly because depth can only be gauged relatively and not by any absolute system of measurements; an object may appear clearly on this or that side of another but of the actual distance involved, the observer may have no notion. Also a surface perpendicular to the line of vision has the same depth throughout, but asymmetrical points on it are unequally distant. Quite different is the case with the term distance; correlative to the several meanings of optical size, we have geometric, estimated, and visual distances and it is the confusion of these terms which has brought confusion into theories of the horizon illusion. As noted above, Emmert's law of the proportionality of distance of projection and size of after-images is based on laboratory experiments where geometric and visual distances are practically equal but is invalid for the "great open spaces"; a good example of the confusion of estimated and visual distances in the open is to be found

in the size-distance or "comparison" theory of the horizon illusion. Of the nine theories of the illusion catalogued by Claperède (6, p. 121), the size-distance view is best known and most commonly adopted. The distance factor has been variously attributed to interposed objects, aerial perspective and the flattened shape of the firmament. Of these, Malebranche (27, Book 1, Chap. 7) adopted that of "interposed objects" and gave it expression in the form which has become traditional. Discussing the effect of perceived distance on the apparent size of a steeple, Malebranche goes on to say: "C'est-pour cela que quand la lune le lève ou elle se couche, nous la voyons beaucoup plus grande que lorsqu'elle est fort élevée sur l'horizon: car, etant fort haute, nous ne voyons pas entre elle et nous d'objets dont nous sachions la grandeur pour juger de celle de la lune par leur comparison. Mais quand elle vient de se lever ou qu'elle prête à se coucher, nous voyons entre nous et elle plusieurs compagnes dont nous connaissons à peu près la grandeur; et ainsi nous la jugeons plus éloignee, et à cause de cela nous la voyons plus grande." Malebranche here gives the size-distance theory of comparison in its purity; the retinal image being constant, the projected images vary with the distance of projection suggested by the landscape. Such a distance is a good example of a *"geschätze Entfernung"*—an estimated distance—the estimation being condensed into a spontaneous judgment expressed in terms of stretches of fields and forests instead of feet or miles. To the objection that the illusion appears at sea where there are no "intervening objects," Malebranche would probably reply that the sea, no less than a field, is an intervening object. In these panoramic effects binocular parallax can play but small part in comparison with the secondary criteria of distance; in addition, monocular vision is here quite as efficacious as binocular.[9]

[9]For some time it seemed as if the question of the primary factors of apparent size had been settled by Hilldebrand, following Hering; depth was determined by binocular parallax, and size was a resultant of retinal image (visible angle) and apparent distance. But, on the basis of ingenious and profuse experimentation, Jaensch concluded that visual magnitude was determined by attention accompanying convergence processes in which binocular parallax was merely the most delicate agent. Using a monocular haploscope, Jaensch found, with increased convergence and increased distance, an apparently smaller stimulus.

In the Aubert-Foster phenomenon where, with equal retinal images for a far and for a near field, a larger proportion of the near field is apperceived than of the far, Jaensch held that the greater convergence for the near field was accompanied by attention processes resulting in an increased

The moon just rising into sight naturally gets a horizon setting, but, when still close to the horizon, it has seemed nearer than when on high to not a few observers. Claparède and Filehne together report the observations of 120 aides of whom 116 found the moon nearest when near the horizon; estimates of the apparent distance were given more often in meters than in kilometers. The lowest term in this series of distances comes from W. Lohmann (26, p. 154) who reports that the moon at the horizon seems to him within reach (*greifbar*).

In its day the comparison theory was held to be disproved by observations like those just noted; the moon seemed farther away and yet smaller. There is, however, no contradiction here, for the conditions as between the moon low and the moon on high are reversed; when low, the *données* for the effect are the everyday space experiences of the landscape—fields, woods, buildings. Not so with the moon on high: no definite space experiences come from gazing into the blue of the sky; the only *donnée* here is the impression of the moon's size through which the distance is judged; the moon seeming smaller than when low, the distance seems larger—a case of so-called secondary illusion of judgment noted in Kaila's investigation.

Several investigators have tried to measure the illusion by comparing the disk of the sun or moon at different altitudes with series of disks graduated in size or with disks viewed at different distances. The results are not in close agreement—an outcome not surprising in view of the differences in experimentation and the large number of variables entering into the problem.

In investigations on the shape of the firmament and the apparent diameter of the sun and moon at different altitudes, R. von Sterneck "measured apparent distances between stars and apparent diameters" by means of a graduated rod which he held in his hand (8, p. 547). To this procedure Alöis Müller (31, p. 52) vigorously objected as well as to the common way of expressing the size of the sun or moon

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vividness (*Eindringlichkeit*) and hence with a larger proportion of the field apperceived. But Kaila (22, p. 211), using an extension haploscope, found the phenomenon intact for both far and near fields, convergence as well as accommodation and retinal image being constant. The case of apparent greater distance with smaller retinal image Kaila regards as an associative effect taking place where the object is well known. Kaila's explanation of these and allied phenomena is based on well-known inhibition processes and is simpler as well as more explicative than Jaensch's rather vague formula of "attention processes accompanying the convergence impulse."

in terms of well-known objects or in centimeters. One may not measure psychical magnitudes (*Sehgrössen*), he declares, with physical units. One may no more measure a visual magnitude with a meter-rod than "strength of feeling in amperes." Without entering into the epistemology of the question or discussing the ambiguity in the use of the word "*Grösse,*" it would seem that Müller is here tilting against a man of straw: when Reimann or Filehne compare the disk of the moon with a graduated series of disks they are, of course, comparing *"Sehgrösse"* with *"Sehgrösse."* If, thereafter, they say the moon appeared to them about the size of a 12-cm. disk 12 meters away, or, directly, that the moon looked 12 cm. across, the difference is merely verbal. Peering through a marsh fog in the early morning, the writer was surprised, if not startled, to see a little yellow disk which immediately suggested "orange-size" and thereafter, reflectively, "That must be the sun." Parenthetically, it may be said that a vision of the sun expressible in citrous magnitudes has not been a frequent experience of the writer. Into the tedious analysis of the reproductive processes leading up to this judgment of size, it is unnecessary to go; it was a valid comparison with an associative tag and akin to "absolute judgments" in discrimination experiments with variable preceding norm. But as against R. von Sterneck's (7, p. 1690) formula equating apparent distance with geometric distance, Müller's objection seems valid; this equation is $d' = \frac{cd}{c+d}$ where d' is the apparent and d the geometric distance, c being a geometric constant depending on the nature of the terrain. The terms of this equation, one psychical, the other geometric, are connected by the sign of equality which here implies mathematical significance—an impossible relation.

When, however, one says that the moon appears to be 20 cm. across or, for short, that it equals 20 cm., there is no equating of heterogeneous values; "is" or "equals" here means "like" or, better, "no perceptible difference," and both terms express psychical values —*Sehgrössen*. On the other hand, applying von Sterneck's formula to the case of a broad plain where c would be at the horizon—say 10 kilometers away—one gets the remarkable result that the length of a kilometer from the observer would appear to be 10/11 of the actual distance, whilst an upright rod 99 cm. high, placed one kilometer away, would seem to be 90 cm. long.

Of more value is von Sterneck's theory of surfaces of reference (*Referenzflächen*) noted above; likewise, it is a curious example of confusion of psychical and mental values. The moon, he says, having a mean angular value of 31′ 8″ appears to be 15 cm. in diameter, hence 16.6 m. away, though no one has the impression that it is only so far off. What is really meant is that the moon would be just eclipsed by a disk 15 cm. in diameter, 16.6 m. from an observer, and this implies that we construct a near-lying surface of reference (*Referenzfläche*) in judging celestial distances. As we have had no practice in translating into linear values the spread of angles having horizon radii for arms, we "prefer" as he puts it, for purposes of comparison, "die scheinbaren Ausmasse am Himmel . . . auf eine, in vernunftiger Entfernung verlaufende Referenzfläche zu beziehen" (7, p. 1703). For von Sterneck, therefore, what he directly saw was not so real as what he supposed he saw; phenomenal space was a shadowy abstraction of geometric space and *"Sehgrösse"* a function of *"geschätze Entfernung."* Presumedly von Sterneck had never visited one of those edifying shows known as cycloramas, where the indefinite depths and vast spaces of a landscape are realistically given in a building of about 100 feet diameter. The immediate foreground of the cyclorama is taken up with "real," that is, with tangible objects—fences, farm-tools, hay-cocks, etc., made on a slightly reduced scale—which are cunningly blended into the depicted section of fields, woods, hills, or distant mountains with their shadowy blue tints melting into the blue of the sky. The illusion is complete, and, for the time one gazes at it, perfect. That is to say, within the 40- or 50-foot radius of the cyclorama one gets the convincing effect of a landscape with a horizon distance of 40 or 50 miles, and, through the insertion of more features suggesting distance than are commonly found in nature, the effect is, so to speak, more "real" than reality. Now the cyclorama is the phenomenal embodiment of von Sterneck's "surfaces of reference," but, while it is the counterfeit presentment of actual spatial experience, "surfaces of reference" are merely a logicised construction of a supposed spatial illusion applied to the sky and horizon. *De facto,* each experience of visual space has its actual, not logicised, limits of a cycloramic character; it is phenomenally real and if measured on the size-distance basis its depth is limited to comparatively few meters. Standing on the strand some 50 yards from the water's edge, the beach seems but a narrow strip in comparison with the indefinite stretch of ocean beyond, but physi-

cally, i.e., retinally, the beach takes up more than 90% of the image; and in the cyclorama, as well as in "seascapes," the ocean covers but a slender strip of canvas. As Mach says, "Visual space is not only limited but seems to have very narrow limits." While each of these experiences of visual space is probably unique, they may, as a whole, be broadly grouped according to the individual's contacts with the external world: thus we have room space and garden space built up in childhood and the street, room, field and mountain spaces constructed from later experience. In passing from one "construct" to another, a process of adjustment often takes place; in a large theater the actors at first seem small, but, as the play goes on, they take on normal size. Marionettes at first appear to have only the 3 or 4 feet of their metric size, but later on they seem to be of normal height. If, then, the hand of the operator comes into view from behind the scenes, it appears gigantic.

Within each of the several regions of space, objects metrically equal appear actually so, without regard to the retinal image: in a drawing-room a man 5 feet away seems about the same size as one of equal height 30 feet distant, but the retinal image of the nearer man has 36 times the area of the farther. In a field, trees of equal height at different distances, with corresponding differences in the retinal images, still appear of like size. *Per contra,* of trees at different distances, giving equal retinal images, the farther will seem larger; with equal retinal images, a square foot a yard away seems much smaller than a 2-foot square 2 yards distant. Hence it is possible to match objects of approximately like size placed at unequal distances from the observer, i.e., with unequal retinal images; thus Götz Martius (28, p. 611) compared a graduated series of slender rods with a standard rod of 20 cm. 0.5 meter away and found approximate equality with the variable, 21.92 cm. long, 5.75 meters beyond the standard. Likewise, a standard rod 100 cm. long, 0.5 meter distant, seemed practically equal to a rod 110 cm. long, 5.80 meters away; the retinal image of the variable being less than one eleventh as long as that of the norm. In general, Martius found a slow decrease in apparent size with increasing distance. A like result was shown by W. Holtz (17, p. 159) using disks instead of rods, though, owing to differences in milieu and conditions of experimentation, the figures are only roughly comparable with those of Martius. Street lights in a row appear sensibly alike and the convergence of parallel rows seems much less than the perspective re-

duction. More striking is the effect in a railway "coach," where the passengers all appear to have their near-by or actual size, despite the strong perspective influence of the long lines of the car.

In familiar space perceptions where we see things in their actual (which probably means their "tactual") size, we have the analogue of the practical effects we get from color impressions: for "human nature's daily use" snow is white, soot is black, gold is yellow, and grass is green, whatever the color or shade reflected from these objects. Shadows of snow-drifts under a yellow sun appear as shadows against white and not blue against a yellowish snow; a sheet of white paper in a shaded recess seems white, though gray if viewed through a tube, and the black of print appears black at noon though reflecting more light than the white paper in the morning. These colors which seem to inhere in the surface of certain classes of familiar objects Hering (16, p. 16) calls memory colors (*"Gedächtnissfarben"*) and he comments on their value for purposes of recognition. For, unless familiar objects had a certain constancy of surface appearance, if they changed shade or tint or both with every variation in the kind and amount of light reflected, the effect would be to change our quick recognition of objects of common experience into processes of slow inspection. Hering's explanation of this constancy of color is mainly physiological, i.e., it is based on pupillary and retinal action and less on reproductive mental processes than is implied in the term *"Gedächtnissfarben."* The extent, however, to which such processes can affect sensory impressions—tints and shades—was ingeniously tested by David Katz (23, p. 517). Most psychologists and landscape artists have remarked the difference between an isolated patch of color and a like patch in a known milieu, and on such experiences Katz based his experimentation. Viewing through a hole in a screen a pair of Maxwell disks, one white in deep shadow, the other "clothblack," Katz found that only 4.2° (average) of white had to be added to the black disk to make it "like" the darkened white disk; but, with the screen removed and the disks viewed in the ordinary setting of a laboratory room, the situation called for 116° of white to be added to the black to give a like shade to the two disks. What the observer saw in the first case was two patches of dark gray; in the second case a dark gray disk and a white disk in shadow, and the 116° minus the 4.2° was the amount of brightening contributed to the second situation by a knowledge of the circumstances, i.e., by central mental processes.

If central processes can so markedly change the values of simple sensory impressions, it is naturally to be expected that they would be still more predominant in the complicated processes of space perception even without the evidence of the many illusions of space; as the values of tints and shades are changed with the falling out of the reproductive processes arising from a milieu, so we find variations in the dimensions of known objects with changes in the extent of their surroundings; viewed through a slit or tube, things appear smaller and in a dark room or field of the closed eyes after-images seem relatively minute, that of a 1-cm. black disk 2 meters away appearing like a pinhead of light (1, p. 576). With vertical vision having a scanty milieu, objects appear in miniature, e.g., cattle in a meadow below a high cliff, but where vertical views are found in range with structures at once lofty and narrow, one gets increased distance effects with usually increased size. In this connection, it is to be remarked that when one gazes slantwise across an ocean beach one "sees" a more distant horizon than when looking straight out to sea. However, in our several constructs of reference—house, garden, field—it is not horizontal vision as such that induces constancy of size; lying on one's side brings about an effect of brighter colors and slightly decreased size. More pronounced is the effect of gazing with head inverted; objects appear decidedly smaller and likewise farther away, the latter probably a "secondary illusion" which connects smaller size with greater distance. But whether the surroundings of an object are cut off physically or cut out by a narrowing of the field of attention, the outcome is similar—a decrease in the size of the object. As Bourdon has noted (5, p. 115), the moon, under repeated or continuous observation, seems to become smaller, i.e., the illusion is lessened; for with attention focussed on the moon's disk the surroundings become inoperative and the moon assumes something of the empty setting it has on high. In general, well-known external objects seem smaller when viewed with exclusive attention, e.g., the books across the room in one's library.

Earlier in this article it was remarked that probably each case of the horizon illusion is a unique experience involving a special explanation. For, under practically like objective conditions, we may get greatly diversified effects not only for different individuals but for the same person at different moments. Correspondent to this multifarious diversity there is naught else in human experience save the boundless variety of reproductive and assimilative processes under

the varying conditions of attention. Whether one views a scene with a fixed or wandering gaze is, physically speaking, a matter of relatively little difference, but, psychically, the difference is one involving the life-long experience of the individual in perceptions of space. Claparède's explanation of the greater size of the moon at the horizon as deriving from our "interest" in the moon viewed as a terrestrial object is, of course, an easy-going way of expressing an attention theory; likewise, it is on the reproductive processes underlying acts of attention that E. R. Jaensch bases his explanation of the paradoxical "laws" of the Aubert-Foster and of the Koster phenomena as well as any of the minor findings of his ingenious experimentation. But whether one adopts Jaensch's views or any other theory of attention—inhibitive, facilitative, or a combination of both—the predominant rôle played by reproductive ideation in the varied phenomena of space perception is something beyond doubt.

When close to a person, our attention is wont to be occupied with details of his face, his manner, or his dress, that is to say, with sensory rather than with reproduced processes, and it is to be observed that the two, together with the accompanying physiological processes, are mutually exclusive. But the further away, the greater is the influence of the reproductive processes on those sensory properties which distance most affects, e.g., size, and just as an object tends to keep its conventional color in all lights, so an object belonging to a familiar class tends to maintain its actual size, i.e., its size as established by perceptions of touch and movement for which the visual impressions are surrogate.

It is evident that the writer is here applying an inhibitory theory of attention which at present seems to account for a fairly wide range of psychical phenomena with closely parallel action on the physiological side. But, whatever the theory, there is no doubt in regard to the phenomena. As before remarked, people in a room are about alike in spatial effects, and while, broadly speaking, objects become smaller with greater remoteness, still actual experience is more powerful than distance in effects of extensity; in a large field we see the far trees as tall as the near, for trees of about equal height. In fact, distance in the sense of remoteness is essentially a physical quantity and psychically an inference. Binocular parallax, whether acting directly (Hillebrand) or as a signal for attention processes (E. R. Jaensch), is mainly useful for arm's-length distances and, in any case, gives only relative depth, but is silent as regards absolute distance.

That we see a mountain 50 miles away does not mean that we see the distance; we see the mountain and infer or measure the distance. A like effect of distance could be produced by a vertical surface 40 feet away. Looking across parallel ranges of mountains separated, it may be, by broad valleys, we get the effect of one ridge. Johannes Lindworsky (25) has proposed to cover over the worst pitfalls in the discussion of space perception by substituting for perceived size (*Sehgrösse*) the terms "power" or "might" (*Mächtigkeit*) of extension in the field of vision, but so far most investigators prefer to go on with the words size and distance with the caveat that they are using each term in one of its three meanings.

To conclude, we construct for ourselves in geometric space as a result of individual experience a series of fields or spheres of reference in each of which objects are approximately of like largeness or "might," and while, with increasing remoteness of a field, a given class of objects has less "might" there is no clear relation between largeness and distance. Perhaps the most important fact in this connection is the generalization of Götz Martius from his experiments on the "apparent size of objects": "One can say that perceived space compared with real space is contracted; the degree of contraction increases with the distance but not proportionally." Applying measurement, if one may apply it to these phenomena, we may say that a moon gauged as 30 cm. across indicates a spatial construct of about 30 meters radius and other largenesses of the moon, resulting from the general experience and special attention processes of the percipient individual, would give proportional values. To quote Mach again "Visual space is not only limited, but seems to have very narrow limits." If such narrow limits as are indicated by the above figures seem nonsense to anyone who has a firm conviction that he can see a distance of 50 miles, the writer would advise a search for some panorama which haply may have survived the movie deluge or failing this, a well-painted theatrical landscape.

The term "horizon illusion" as popularly used means an apparent enlargement of the disks of the moon or sun when rising or setting above their size when on high which is regarded as their standard or "real" size; it does not mean that what we see as shining disks are "in reality" huge spheres astronomical distances away. Psychologically, there is more reason for attributing standard or "real" size to these bodies when on high than at the horizon and *de facto,* in some cases, the horizon size has been regarded as standard. But,

according to the view adopted here, the horizon moon is one of a series of spatial constructions of which the home, the garden, and the field are more intimate members. Accordingly, we may regard the rising sun or moon qualitatively as much or as little illusory as the largeness of persons in a room or as of trees in a field.

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L'ILLUSION DE L'HORIZON: II

(Résumé)

Les investigateurs (dont la plupart sont des physiologistes) donnent des description différentes et contradictoires de la grandeur et de la distance apparentes des corps célestes et de la forme et des dimensions du firmament.

On ne considère pas comme il faut les procédés psychiques en jeu. Tandis que la contraction du champ de vision décroît la grandeur et la distance de l'objet vu pour un champ donné, les procédés de l'attention sont pour la plupart déterminatifs des proportions de ces propriétés.

Les investigations de Jaensch ont montré le rôle de l'attention dans les effets haploscopiques et dans les phénomènes de profondeur. Les observations des images du coucher du soleil lesquelles viennent après indiquent le jeu de l'attention dans la détermination de la grandeur et de la distance apparentes de l'image.

Dans ces expériences et celles de pareille nature c'est la position entière d'un objet laquelle détermine les propriétés spatiales et non la comparison avec des objets spécifiques.

Le résultat de l'expérience quotidienne avec la vision normale (droite) montre que le champ est approximativement divisé en zones—maison, jardin, ou campagne, avec une lente décroissance de la grandeur apparente comme la distance s'accroît mais avec une constance apparente de la grandeur pour les objets dans une zone donnée. Les personnes dans une salle paraissent d'à peu près la même grandeur et de même au cas des arbres dans un champ. Cette constance est analogique à la constance des couleurs rappelées dans la perception des couleurs. Le champ entier de la vision est petit comme montré réalistiquement dans le rayon de quarante pieds d'un "panorama"; une lune qui a apparemment un diamètre de trente cm. indique un champ d'un rayon approximativement de trente m.

Dans la plupart des investigations de la soi-distant illusion de l'horizon, l'implication physique des terms "grandeur" et distance" a rendu la discussion confuse.

ANGELL

DIE ILLUSION DES HORIZONTES

(Referat)

Es werden von verschieden Forschern,—meistens Physiologen,—verschiedene und widerstreitenden Berichte erteilt über die augenscheinliche Grösse und Entfernung der Himmelskörper und über Form und Dimensionen des Firmaments.

Die mit einbegriffenen psychologischen Vorgänge werden nur streifend berücksichtigt. Obwohl Einschränkung des Gesichtsfeldes die Grösse und die Entfernung des besichtigten Gegenstandes in irgend einem besonderen Felde vermindert, werden die Proportionen dieser Eigenschaften grossenteils durch Aufmerksamkeitsvorgänge bestimmt.

Untersuchungen von Jaensch haben die Rolle der Aufmerksamkeit bei haploskopischen Erscheinungen, und Erscheinungen, bei denen die Wahrnehmung der Tiefe mit einbegriffen ist, erwiesen. Beobachtungen an Nachbildern der untergehenden Sonne weisen auf Bewegung der Aufmerksamkeit hin, bei der Bestimmung der augenscheinlichen Grösse und Entfernung des Bildes.

In diesen und verwandten Versuchen werden räumliche Eigenschaften

durch die allgemeine Inszenierung (setting) eines Gegenstandes, und nicht durch Vergleichung mit spezifischen Gegenständen bestimmt.

Die Ergebnisse der alltäglichen Erfahrung mit normalem (aufrechtem) Sehen (vision erweisen, dass sich das Gesichtsfeld im grossen Ganzen in Zonen enteilen lässt, -Haus, Garten, Landschaft, u. s. w.). Die augenscheinliche Grösse dieser Zonen vermindert sich mit der zunehmenden Entfernung. Die Grösse von Gegenständen innerhalb einer bestimmten Zone bleibt aber augenscheinlich konstant. Menschen in einem Zimmer scheinen von ungefähr gleicher Grösse zu sein, und ebenfals Bäume in einem Felde. Diese Konstanz zeigt Analogien mit der Konstanz von Erinnerungsfarben (memory colors) (Hering) bei der Wahrnehmung von Farben. Das ganze Gesichtsfeld ist klein, wie an dem 40-füssigen Umkreis eines Panoramas realistisch gezeigt wird. Ein Mond mit einem augenscheinlichen Radius von 30 cm. weist auf ein Feld hin, dessen Radius ungefähr dreissig Meter beträgt.

In der Mehrzahl der Untersuchungen der sogenannten Illusion des Horizontes (horizon illusion) ist die Besprechung durch die physikalische Mitbezeichnug (connotation) in den Wörten 'Grosse,' 'Entfernung' verwirrt worden.

ANGELL

THE PSYCHOLOGY OF ARISTOTLE: A LOGICAL ARRANGEMENT OF HIS *DE ANIMA* AND ALLIED TREATISES*[1]

(In Synoptic Form)

Christ Loukas

Part I

By way of an introduction, it may be said that to a few admirers of Aristotle it will forever appear an excellent thing that the discussion in his psychological treatises is variously interrupted and frequently turned in new directions without an always evident reason, as is often true of any great teacher who pauses to repeat, to bring in historical example, to press home a point against opposing doctrines, or to take up the argument from a new approach. But for the majority of the readers, the obscurities of his *De Anima* and other treatises related to it are undoubtedly increased by what appear somewhat serious defects of composition. Aside from certain omissions and passages repeated or evidently misplaced, such as one should expect in ancient writings, there is a great want of logical connection between the various divisions of his treatises, which is almost certain to give rise to perplexity, especially to students not familiar with his methods of reasoning.[2] For this reason the writer has attempted to bring together in this article, in a rather summary form, to boil down as it were, the main theses of Aristotle's psychological argument,[3]

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[1]This article is the outgrowth of a former paper on the Greek psychology, presented by the writer in Dr. E. S. Conklin's graduate seminar.

[2]For criticism of Aristotle's organization of material, see notes in Hick's *De Anima* (14, pp. 173-588), Ross' *Aristotle, De Sensu et de Memoria* (25, pp. 1 and 33), and Lewes' *Aristotle* (19, pp. 108 ff.).

[3]In preparing this article, the writer had before him both the Greek original and the translations of Aristotle's psychological treatises as indicated in the attached bibliography. In quoting from these translations, he did not in every case use complete sentences or paragraphs from any one writer, but often substituted words, phrases, or clauses which in his opinion seemed to render more accurate the interpretation of Aristotle's theories. The writer, therefore, assumes all responsibility for alterations in these translations made for the sake of clarity.

leaving the reader to trace at will (in the sources quoted) his refutation of the theory and contentions of his predecessors and contemporaries.

His Life. Aristotle was born in Stagira in the year 384 B.C. His father belonged to the Asclepeadae guild, was the physician and friend of Amyntas II of Macedonia. He lost his father at the age of seventeen and his mother at an earlier period. But the loss of his parents was supplied and compensated for by the kind attention of Proxenous, who received him into his family and skilfully directed his education.

At the age of eighteen Aristotle was attracted to Athens by the love of learning, and particularly by the desire to hear Plato, under whom he studied for twenty years. Plato often called him the "soul of the school" and named him "the reader par excellence." In 343 B.C. he became the tutor of Alexander the Great, who later aided him in his scientific research. Alexander respected Aristotle so much that he is reported as saying "that he honored Aristotle no less than his father, because if to the one he owed life, to the other he owed that which made life valuable." In 335 B.C. he returned to Athens and opened a school in the Lyceum, a gymnasium in the vicinity of the temple of Apollo Lykeios. He was wont to converse with his scholars as he walked up and down in the gymnasium between the rows of trees, and from this custom his school derived the name of the Peripatetic. His forenoon lectures were more of a scientific nature and were confined to a select audience, e.g., to students. His afternoon ones were popular in form and were open to the public at large.

On the death of Alexander (323 B.C.) Athens became the center of an outbreak of anti-Macedonian feeling, and Aristotle's Macedonian connections made him an object of suspicion. An absurd charge of impiety was brought against him, based on a hymn, written to Hermias, in which he extolled virtue above all earthly possessions, and an epigram (which was engraved on his statue at Delphi) in which he related the unworthy and treacherous death of Hermias. Determined not to let the Athenians "sin twice against philosophy," he left the school in the hands of Theophrastus and withdrew to Chalchis, where, in the summer of 322 B.C., in the sixty-third year of his life, he succumbed to a disease from which he had long suffered. He is said to have written his own defense and to have inveighed in strong metaphor against the increasing degeneracy of the

Athenians. Since he escaped his trial, his discourse was not delivered in court.

According to Diogenes Laertius, Aristotle composed over four hundred different treatises, of which only forty-eight have been transmitted to the present age. His literary work may be divided into three main sections. The first section consists of works of a more or less popular order (exoteric) which were published by himself; the second, of memoranda and collections of material for scientific treatises; and the last, of scientific works proper. Apart from the "Athenaeon Politia," the whole existing copies of his works, so far as they are authentic, belong to the third class.[4]

Due to the fact that Aristotle approaches his subject of psychology from the side of metaphysics,[5] it is deemed expedient to include a brief outline of his metaphysics, which may serve as a background for the subject-matter that is to follow.

Metaphysics. "The motive which inspires Aristotle throughout the Metaphysics is the wish to acquire that form of knowledge which is most worthy of the name of wisdom. The desire to know, he points out, is innate in man. It is seen, at the lowest level, in the delight we take in the use of our senses. The first stage above this in the direction of completer knowledge is that involved in the use of memory, which distinguishes us from the lowest animals. The next stage—and one which only man attains—is experience, whereby through the coalescence of many memories of the same kind of objects (e.g., of what helped Calias and Socrates and others when ill of certain diseases) we acquire, without knowing the reasons for it, a rule of practice. One stage higher is art, the knowledge of practical rules resting on general principles. Highest of all comes 'Science,' the pure knowledge of causes; this is highest because it is not, like art, limited in its interests by having some ulterior practical end, but is knowing for Knowing's sake. This is the last and highest product of civilization."[6]

" 'Wisdom' must be not only science or knowledge of causes, but knowledge of the first and most universal causes. For this satisfies most completely the criteria of wisdom we should naturally use. It

[4]Hicks (17, Vol. 1); Gillis (13); Grote (14, Vol. 1, Chap. 1); Lewes (21, Chap. 1); Pillsbury (25); Ross (29, Chap. 1); Zeller (37, Vol. 1, Chap. 1); Loukas (22).

[5]Brett (6, Vol. 1, p. 100).

[6]Ross (28, Book 1, Chap. 2). Quoted from (29, p. 154). See also Zeller (37, Vol. 1, Chap. 7).

is the most comprehensive knowledge; the knowledge of what is hardest to know, since its objects, being the most universal, are the farthest from sense; the most precise knowledge, since its objects are the most abstract, the least complex; the most instructive; the most self-contained or independent; and the most authoritative, since it will be *interalia* knowledge of the final cause of all things. Philosophy springs from primitive wonder, and moves towards the abolition of wonder, towards understanding the world so well through and through that no room is left for wonder at things being as they are."[7]

The first principle or primal substance, for Aristotle, is that which is in no other but which all other is; it can be predicated of no other, but all other is predicated of it. This individual is the first essence; species is a "second essence," the quiddity, common to many individuals.[8]

The two constitutive principles of any object are matter and form. The former is identical with potentiality, the latter with actuality.[9] The particular objects or individual beings are real substances. The true nature of the particular concrete being is constituted by its form, by its general qualities belonging to the class to which it belongs. The particular object either changes or grows. Whatever is perceived is changeable, it is or is not, it can be or not be;[10] it assumes now these qualities, now those; it is now seed, now sapling, now tree, now fruit. The thing that changes or persists in this changing process is matter. Matter itself cannot disappear, it cannot be experienced as formless, it is always coexistent with form. Form never changes, but it is matter that assumes different forms, or a series of forms, e.g., one form following another.[11] Its initial form does not change into another form, but a new form fashions the matter. The different forms do not come into being suddenly; they have always existed. And, since matter and form coexist, it follows that neither the one nor the other arises or disappears, and hence they are the two eternal principles of things.[12]

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[7]Ross (28, Book 1, Chap. 2); Ross (29, pp. 154-155).

[8]Ross (28, Book 7, Chaps. 1-4).

[9]"There are two meanings of actuality: knowledge illustrates the one, exercise of knowledge the other" Hicks (16, Book 2, Chap. 1); Ross (28, Book 11, Chap. 9); Zeller (37, pp. 340-345).

[10]Ross (28, Book 9, Chap. 3).

[11]A comprehensive summary of Aristotle's account of Matter and Form will be found in Ross (29, pp. 167-173).

[12]Ross (28, Book 1, Chap. 9; Book 2, Chap. 2; Book 3, Chap. 6; Book 12, Chaps. 1-12); Thilly (31, p. 83); Zeller (37, Vol. 7, Chap. 9).

Entelechy. When a thing completes its growth, it realizes its aim, its meaning, or form. The form is its true being, its realization or completion (entelechy). Its possibilities have been realized; that which was potential in it has become actual. Matter has taken on form; the acorn becomes an oak. The acorn is a potential oak; the oak is the realization of its potentiality, it is the form made manifest, real, actual. Matter, therefore, is the principle of possibility, and form is the principle of reality or actuality. The seed is matter for the oak; the marble, matter for the statue. Form realizes itself in the thing; it causes matter to move; an end or purpose is realized by the thing.

Four principles can be distinguished in this process, four kinds of causes,[13] namely,

1. Material (that from which a thing is made).
2. Formal (the idea or form of the thing which is to be made).
3. Efficient (that through which it is made).
4. Final (that for the sake of which it is made).

A word of explanation on each of these may be of service.

1. *Material Cause* is the matter itself conceived apart from its form. It is the indeterminate, the becoming, the potential of whatever is produced, in itself unknowable and without predicates. It underlies all change or becoming.

2. *Formal Cause* is the inner and immanent principle of determination. It constitutes the actuality[14] of that which has potential existence. It is the essence of a being—essence which is the cause of all existence. Matter and form constitute substance. These two are always coexistent, the one potentially, the other actually. Matter is the ground of multiplicity. Form is limited by matter, e.g., different forms are actualized in different matters. The ground-matter exists as opposed to form, as privation, which may make a third principle of substance.[15]

3. *Efficient or Moving Cause,* in general, is the combining of matter and form, the transition from the potential to the actual. Its ground is not in matter, for it is an actuality, though imperfect. Form or actuality is prior to matter, at least in thought; but, on the

[13]Ross (28, Book 5, Chap. 2); Thilly (31, pp. 83-84) Enfield (12*a*, p. 159); Zeller (37, Vol. 1, pp. 355-360).

[14]For detailed and comprehensive discussion of Aristotle's distinction between the term potentiality and actuality, see Ross (29, pp. 176-178); also Zeller (37, p. 342).

[15]Ross (28, Book 5, Chap. 2; Book 11, Chap. 9); Lewes (21, p. 128).

other hand, matter is the eternal principle of things, and motion is eternal.[16]

4. *Final Cause.*[17] This gives to every movement an aim, and a benevolent aim.[18] Every becoming has a design, and this is the first cause, and knowledge of it is the highest aim of philosophy. It is, at least in generation, the form, a perfect entelechy, having its end and completion in itself.

"These four causes comprise Nature; for everything has substance, everything has form, everything has motion, everything has an end or aim."[19]

Psychology. Students of modern psychology, whenever they write a treatise on psychology, usually start with the nervous system. Aristotle, however, commenced with the soul. The reason for this is that nerves were unknown to him. To the brain of an organism is given an inferior position because of its location. Aristotle thought the brain was a gland for the secretion of tears and that its only function in connection with mind was to cool the animal spirits when they became too warm, as in strong emotion. It has no blood sent to it; only to the enveloping membranes is a small quantity sent in order to temper its coolness. Hence bloodless animals have no brains, since their moderate heat requires no cooling.[20] The heart, however, occupies the noblest position and has an a priori claim to be regarded as the seat of the noblest function. "All the senses," says Aristotle, "pass through ducts to the heart," but how they pass he does not show.[21]

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[16]*Physics* (258a4), quoted from Ross (30, p. 100).

[17]"The term final cause," says Lewes, "has two meanings: it implies that for which and that by which any result is obtained" (21, p. 234).

[18]Lewes (21, p. 118); Ross (28, Book 12); Ross (29, p. 175).

[19]Lewes (21, p. 129).

[20]Dr. Ogle gives the following reasons why the brain was rejected by Aristotle as the organ of central sense:

1. "Because the brain is insensible to external mechanical stimulation.
2. "Because he could find no brain or anything analogous to a brain in any of the invertebrates except in the cephalopods. Yet sensation was the special characteristic of an animal. The absence of a brain, then, from numerous sentient creatures, was quite incompatible for him with the notion that the brain was the central organ of sensation.
3. "Because he erroneously regarded the brain bloodless and all experience taught him that those parts alone were sensitive that contained blood.
4. "Because he thought it manifest to inspection that there is no anatomical connection between the brain and sense-organs.
5. "Because he believed himself to have good grounds for supposing another part, viz., the heart, to be the sensory center" (2, pp. 330 ff.)

[21]Pillsbury (25, p. 33); Brett (6, Vol. 1, p. 106); Lewes (21, p. 180).

The reader of Aristotle's psychology is impressed from the outset, on the one hand, by the fullness and variety of the material, and, on the other, by the somewhat systematic form into which it is thrown. His investigation concerning the soul deals, as will be seen in the pages that follow, in the first place, with the phenomena of life. Wherever a thing, throughout the changes which it undergoes, or in its inner invisible force, develops toward a goal, there a soul is at work, i.e., a vital principle.[22]

The Soul. The Greek word *psyche* is untranslatable in modern languages. It means more and less than the English word *soul.* More, as having on one side, at all events, a directly physical connection; less, as not in itself implying any religious association. *Vital Principle* comes nearer representing the meaning of *psyche* than the word *soul.*[23] "The soul," for Aristotle, "is the actuality, *entelecheia,*[24] of a natural body having in it the capacity of life."[25]

Biological Nature of the Soul. "Aristotle regards the soul as a substance; it is capable of being a subject of predication, and many phenomena seem to require some concept of soul for their explanation. There is, therefore, no tendency in Aristotle to regard the soul as unreal; but he is perfectly clear that the sciences deal with phenomena, with activities, their nature, origin and purpose."[26] "He has also tried to make his notion clear by the statement that the soul is the form; the body, the matter. Without form matter has no meaning, but the form is not apart from the matter, is probably not necessarily before the matter, and would also be impossible without it. The two are together; each is necessary to the existence of the other. While the form must be given the leading part, it does not control matter."[27] "Soul and body cannot be defined out of relation to each other; a dead body is properly only matter; for the soul is the essence, the true being of that which we call body. It is from this point of

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[22]Dessoir (12, p. 16).

[23]Lewes (21, pp. 221-223).

[24]The expression *entelecheia,* says Zeller, has a double sense: at one time it is the power of an action that is understood by it; at another the activity itself. . . . The soul can be called *entelecheia* only in the former sense, seeing that it is present even in sleep; this is what is meant by the addition "first soul" when (in Book 2, Chap. 1:27) it is said the soul is the first actuality of a natural body having in it the capacity of life (37, Vol. 2, p. 3, footnote).

[25]*De Anima,* Book 1, Chap. 1; Ross (30, p. 200).

[26]Brett (6, Vol. 1, p. 102); *De Anima,* Book 2, Chap. 1.

[27]Pillsbury (25, p. 27); Schwegler (30*a*, p. 149).

view that Aristotle compares the soul to the functional value of an instrument: the soul is to the body what the power of cutting is to the axe: it is its functional reality. . . . . He classifies substances as to form and matter and the combination of form and matter. These are the two ultimate classes or types of reality. . . . . The actual sphere of psychology is the combination of matter and form, body and soul. Here the soul is entelechy; primarily it is a 'first entelechy,' a resident possibility of psychic activities. This can be explained by an analogy: an eye has the power of sight even when it is looking at nothing; man has the power of active thinking even when asleep; and similarly the soul is a 'first entelechy' when regarded as the potentiality of its own full activity. In technical language, the 'first entelechy' is mere possession of soul-powers, while the second entelechy is their actuality." "Consequently, the nature of the soul can be reached through a study of its functions."[28]

Soul in All Living Forms. Plants and animals have souls but not like that of man. The degree of their differences will be shown below.

"All things that are known can be arranged on a scale ascending from pure matter to pure form. The objects of scientific knowledge are always a mixture of matter and form; and position on a scale of being is determined by the degree to which form is realized. The inorganic occupies the lowest place; then comes the organic, which realizes a unity and exhibits activities in accordance with its unitary nature. . . . . Proceeding upward from inert matter we arrive at a point in the scale of being at which matter presents itself in organized forms. The mark of an organism is its power of self-maintenance, its capacity for initiating action which takes effect on the environment. . . . . In man this power is threefold: he absorbs nutriment, perceives objects, and transcends the immediate moment of perception in thought."[29]

Parts of the Soul. "The above three modes of activity are called souls, namely, the nutritive, the sensitive and the rational."[30] "The plant possesses only the first, the animal the first and second, while man possesses all three."[31]

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[28]Brett (6, Vol. 1, pp. 102-103); *De Anima,* Book 2, Chap. 1.
[29]Brett (6, Vol. 1, pp. 100 ff.); Pillsbury (25, p. 26); *De Anima,* Book 7, Chap. 5.
[30]Brett (6, Vol. 1, pp. 102-103); *De Anima,* Book 2, Chap. 1).
[31]Klemm (20, p. 49); *Nichomachean Ethics,* Book 1, Chap. 13; Book 6, Chap. 2; *Politics,* Book 4, Chap. 14.

1. *The Nutritive Soul,* or the first soul—the first vital principle—which is capable of generating another like itself. It is the first and most widely distributed faculty in virtue of which all things possess life. Its functions are reproduction and assimilation of nutriment. It is removed from the sphere of virtue.[32]

2. *The Sensitive Soul,* or the second in ascension from matter, distinguishes pain and pleasure; is affected by the sensible, the particular, the phenomenal, by which the soul is impressed with the sensible forms, as the wax of the seal. In other words, it is the reporter and forerunner for the rational soul.[33]

3. *The Rational Soul* is found in man only, its aim is action—it contains the others as powers, capable of controlling the sensitive but not the nutritive.[34]

"If we look at these three grades of being, the plant, the animal, and man, we see that the soul, insofar as it is common to all three, must be defined as 'the first complete attainment of form on the part of the aggregate of matter.' "[35] And in this definition Aristotle is answering the question, when and where does the soul begin? "Cause and origin have several significations, for the vital principle is equally a cause according to any one of the three defined modes of causation; first, as that whence motion proceeds; second, as that for which motion is produced; third, as the essence of living bodies."[36] Matter, therefore, does not come to life by form. A statue is matter and has form but obviously it has no soul. "It is only such material aggregates as are potentially living, such as are destined by their nature to realize themselves in that form of being which is life. Life therefore, is a term not co-extensive with being or motion; the universe contains that which can and, that which cannot realize life or have life for its realization; the universe therefore is not itself a living whole."[37]

"For the present it may suffice to say that the soul is the origin of the functions above enumerated and is determined by them, namely, by capacities for nutrition, sensation, thought, and by motion.

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[32]*De Anima,* Book 2, Chaps. 2-4; Book 3, Chap. 4; *Nichomachean Ethics,* Book 1, Chap. 13; Lewes (21, p. 235).

[33]*De Anima,* Book 2, Chaps. 2, 5, 6, and 12; Lewes (21, pp. 235 ff.).

[34]*De Anima,* Book 2, Chap. 3; Lewes (21, pp. 234 ff.); *Politics,* Book 7, Chap. 14.

[35]Brett (6, Vol. 1, p. 101); *De Anima,* Book 2, Chap. 1.

[36]Lewes (21, p. 234).

[37]Brett (6, Vol. 1, p. 102).

But whether each one of these is a soul or part of the soul, and if a part, whether it is only logically distinct or separable in space also is a question, the answer to which is in some cases not hard to see; other cases present difficulties. For, just as in the case of plants some of them are found to live when divided and separated from each other (which implies that the soul in each plant, though actually one, is potentially several souls), so, too, when insects or annelida are cut up, we see the same thing happen with other varieties of soul; I mean each of the segments has sensation and moves from place to place, and if it has sensation, it has also imagination and appetency. For where there is sensation there is also pleasure and pain; and where these are, desire also must of necessity be present."[38] In another place, Aristotle considers the soul as the place of forms. "It has been well said that the soul is a place of forms of ideas; except that this is not true of the whole soul, but only of the soul which can think, and again that the forms are there, not in actuality but potentially."[39]

The soul has the following three qualities:[40]

1. *Emotions,* such as anger, placability, love, hatred, fear, confidence, shame, shamelessness, benevolence and its opposite selfishness, compassion, virtuous indignation, envy, emulation and contempt.[41]

2. *Faculties*—these are gifts of nature, such as prudence, wisdom, intuitive reason, intelligence.[41]

3. *Moral states* are nothing more than virtues, and "a virtue is a state of deliberate moral purpose, consisting in a mean that is relative to ourselves, the mean being determined by reason or as a prudent man would determine it. . . ."[42] "Every virtue of excellence has the effect of producing a good condition of that which it is a virtue or excellence, and of enabling it to perform its function well. The excellence of the eye makes the eye good and its function good. Similarly the excellence or virtue of a man will be such a moral state as makes him good and able to perform his functions well."[43] A group of particular virtues are listed herewith as exemplification of the mean state.

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[38] *De Anima,* Book 2, Chap. 2.
[39] *De Anima,* Book 3, Chap. 4.
[40] *Nichomachean Ethics,* Book 2, Chap. 4.
[41] *Nichomachean Ethics,* Book 2, Chap. 4; see also *Rhetoric* (34, Book 2, Chaps. 2-11; Book 6, Chaps. 5-13).
[42] *Nichomachean Ethics,* Book 2, Chap. 6.
[43] *Nichomachean Ethics,* Book 2, Chap. 5.

Excess	Mean state	Deficiency
Foolhardiness	Courage	Cowardice
Licentiousness	Temperance	Insensibility
Prodigality	Liberality	Illiberality
Vanity	Highmindedness	Littlemindedness
Ambition	(No mean state)	Lack of ambition
Passion	Gentleness	Impassivity
Boastfulness	Truthfulness	Self depreciation (or irony)
Buffoonery	Wittiness	Boorishness
Obsequiousness (if disinterested. If interested, Flattery)	Friendliness	Quarrelsomeness
Bashfulness	Modesty	Shamelessness[44]

Senses. Aristotle makes the claim "that there is no other sense distinct from the five, by which I mean sight, hearing, smell, taste and touch."[45] Each of them is amply treated in the *De Anima.* The objects of sight, smell, and taste are discussed at great length in the *De Sensu*[46] without making much mention of hearing and none of touch.

Touch is the first and most important of the senses. It is the sense of the tangible. Whether the organ of touch is the flesh is difficult to determine. Yet flesh is only a medium and the essential organ must be something different and internal.[47] Is, then, the sentient organ within the flesh, or is it the flesh itself which is immediately perceptive? No indication can be obtained from the fact of sensation being simultaneous with tangible impression, for were anyone to extend a membrane over his flesh, the part would be equally sensible when touched, and sensible at the moment of contact; and yet, clearly, the sentient organ cannot be in that membrane. When the sentient organ itself is touched, no sensation can there or then be produced, any more than a white object can be seen when placed immediately over the surface of the eye; and thus it is evi-

[44] *Nichomachean Ethics,* Book 2, Chaps. 7-9; Book 3, Chaps, 9-15; Book 4, Chaps. 2-15; Book 8, Chap. 15.

[45] *Nichomachean Ethics,* Book 3, Chap. 1.

[46] "*The Sense and its Objects* is not merely a treatise on the subjects referred to in the title, but takes in also an account of the organs of sensation not an account of each organ in detail but of the general character and ultimate constituents of the sensitive members" (27, p. 2).

[47] *De Anima,* Book 2, Chap. 10; *De Sensu,* Chap. 2; Lewes (21, p. 235); Brett (6, Vol. 1, p. 110).

dent that the part perceptive of tangible impressions must be internal.[48]

"*Taste* is analogous to touch in being a sense whose medium is a part of the organism; that is, its medium and organ is the tongue. The object of taste is a species of tangible; it also has for its object not only that which can be tasted but the tasteless. But since the object of taste is moist, the sense-organ which perceives it must be neither moist nor yet incapable of becoming moist. For taste, then, which needs to be moistened, must have the capacity of absorbing moisture without being dissolved, while at the same time it must not be actually moist. A proof of this is the fact that the tongue has no perception either when very dry or very moist. In the latter case the contact is with the moisture originally in the tongue just as when a man first makes trial of a strong flavor and then tastes some other flavor; or as with the sick to whom all things appear bitter because they perceive them with their tongue full of bitter moisture."[49] The medium and organ of taste, then, is the tongue.

"*Smell* comes midway between touch and taste on one side and hearing on the other, i.e., it comes between those that are forms of touch and those that require a medium capable of overcoming the difficulty of distance. The organ of smell is the nose (or its analogue, the olfactory passages in birds) in the case of animals that breathe; it is constituted of air and smelling occurs in inhalation. In the case of the fish the process is the same, but gills and water are the organ and element respectively. The medium, then, is air (or water) conveyed into the channels of the nose (or gills in the case of fish), and then producing the sensation by means of the conatural spirit. The stimulus then reaches the heart. . . . . There are species of odours. Some are pleasant accidentally, such for example as become pleasant through hunger when the animal is pleased by the smell of food; others are pleasant in themselves, as the smell of flowers."[50]

Hearing has the peculiarity of being a sense which mediates between teacher and pupils, for example. The organ of hearing (the ear) is composed of air. The medium of sound is the external

[48] *De Anima,* Book 2, Chap. 2; Lewes (21, p. 236).
[49] *De Anima,* Book 2, Chap. 10; *De Sensu,* Chaps. 4 and 5; Brett (6, Vol. 1, p. 111).
[50] Brett (6, Vol. 1, pp. 111-112); *De Anima,* Book 2, Chap. 9; *De Sensu,* Chaps. 4 and 5.

air. The process of which hearing is the result is a change in the medium produced by either the collision of two sonant bodies, or by the purposive expulsion of air through the larynx. . . . The shape of the ear enables it to act like a funnel, conducting the moving medium to the point at which the spirits natural to animals are also in motion. Thus there is a complete chain of movement from the first impulse given by the object up to the soul, the centre at which there is conscious realization of sensation. . . . . Sounds are divisible into classes vocal and non-vocal; the vocal can be subdivided according as they are or are not significant.[51]

Sight or vision is an actualization of a medium which in darkness exists potentially. . . . The object of vision is determined from the point of view of sight and each sense has a specific object. Color is neither the object nor the medium of sight, inasmuch as vision is not color. The diaphanous becoming actual constitutes light; color depends on light as light depends on the diaphanous; color is that qualification of the light which is propagated through the diaphanous to the eye—it is therefore the true object of sight. The soul perceives what the eye sees by the following process. Since the eye consists of water, this is transparent (diaphanous) and thus the external and the internal media are alike. This water is supplied to the eye and through that inward until it results in vision.[52]

Common Sense. Each sense is perceptive of its own objects, is innate in its own organ to discriminate qualities—sight, judging of black and white, taste of bitter and sweet. But how do we perceive that qualities differ? Evidently, by some sense, because the impressions are sentient, and the flesh cannot be that final sentient, since to judge of qualities it must of necessity first touch bodies.[53]

In the third book (Chapter One) of his *De Anima,* Aristotle states categorically that there are only five senses, that each sense can only discriminate its own objects, that that which perceives white to be different from sweet cannot be the sense of taste nor the sense sight, yet it must be a sense, because the impression is sentient; finally, he says that the sense cannot be touch, because, to judge of qualities, that sense must first touch bodies—by which he probably

[51]Brett (6, Vol. 1, pp. 112-113); *De Anima,* Book 2, Chap. 8; *De Sensu,* Chaps. 1 and 3; Lewes (21, p. 251).

[52]Brett (6, Vol. 1, p. 116); *De Anima,* Book 2, Chap. 7; *De Sensu,* Chaps. 3 and 6; Lewes (21, p. 246).

[53]Lewes (21, pp. 238-239); *De Anima,* Book 3, Chap. 2; *De Sensu,* Chap. 4.

means that white and sweet, not being tangible, cannot be perceived by touch. By this judicial sense he means the common sensorium, which is the front center (heart) of each animal.[54] In other words the senses of an organism are related to each other and sense objects together, and each sense is conscious of itself. This relation of unification, or the sensing of objects together, is due to an additional sense which is common to all. He classes the objects of common sense into common and incidental sensibles.[55]

Perception. "It must be admitted that each sense is receptive of the sensible forms of things (ideas, images) without their matter, as wax takes the impress from a seal-ring without the iron or gold of which the ring is made. So too, a sense, as relative to each sensible is affected by that which has color or flavor or sound. . . The primary sense-organ is that in which such power is resident. The organ is the same as the faculty, but its being is different; for that which perceives must be a spatial magnitude of some kind, but a thing's being receptive, i.e., sense itself is not a magnitude, but a certain ratio or power in the organ."[56]

Part II

"*Memory*[57] concerns the past and not the present. No one would say that he remembers that the present is present, e.g., this particular white object when he is looking at it. Nor would he say that he remembers that the object of thought is present whensoever he chances to be engaged in thought or contemplation; in the one case, he says he perceives; in the other, merely that he knows. But when knowledge or perception is present without actual experience of the real objects, in those circumstances one remembers in the one case that he learned something or thought of something; in the other, that he heard, or saw, or had some similar sense experience, when one actually remembers he had heard or perceived or thought of the thing remembered. Hence memory is neither perception, nor

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[54]*De Anima,* Book 3, Chap. 2; *De Sensu,* Chap. 7; Lewes (21, p. 239); Brett (6, Vol. 1, Pp. 120-124); Pillsbury (25, pp. 29 ff.); Ross (27, pp. 14 ff.).

[55]*De Sensu,* Chap. 4 (426 5-7).

[56]*De Anima,* Book 2, Chap. 12; *De Sensu,* Chaps. 6 and 7.

[57]"Memory is used in a very restricted sense, one much narrower than that assigned to it by modern psychology. It does not comprise retention; that rather is an element present in the general facility of Imagination of which Memory is a special determination" (27, p. 34).

conceptual thought, but some permanent condition or modification attaching to them dependent upon lapse of time. What is now present we do not now in present time remember . . . . , with the present, perception is employed, with the future, hope, with the past, memory. Hence all remembering implies lapse of time; and so those that have a sense of time are the only animals that remember, and the organ of memory is that which enables us to perceive time."[58]

"Memory belongs to the same psychic faculty to which imagination must be assigned." . . . . . "To the class of objects of memory *per se* belong all things that can be imagined; to the indirect, all that cannot be divorced from imagination." . . . . . "A difficulty might be raised as to how it can ever come about that, though contemporaneously with our present mental modification the real object is not present, yet it is the absent object which is remembered. But this is no impossibility, for it is clear that we must regard the modification arising from sensation in the soul, and in that bodily part where sense resides, as if it were a picture of the real thing, and memory we call the permanent existence of this modification. When a stimulus occurs, it imprints as it were a mould of the sense-affection exactly as a seal-ring acts in stamping."[59]

"This is the reason why memory does not occur in those who are in a rapid state of transition, whether owing to some perturbing experience or their period of life; it is as if this stimulus, like the seals, were stamped on running water. Again in others their worn-out condition—like that of an old building—and the hardness of the receptive structure, prevent the sense-affection from leaving an impression. Hence we explain why the very young and the aged have no memory; in the former growth, in the latter decay, cause rapid transition. In like manner, also, both those who are too quick and those who are too slow have bad memories. The former are too moist, the latter too hard, so that in the case of the former the presented image does not remain in the soul, while on the latter it is not imprinted at all. . . . ."[60]

" . . . As regards the question, therefore, what memory or remembering is, it has now been shown that it is the state of a presentation, related as a *likeness* to that of which it is a presenta-

[58] *De Memoria,* Chap. 1 (449*b*-15-34) (Trans. by J. L. Beare and G. R. T. Ross).
[59] *De Memoria,* Chap. 1 (450*a* 25-34).
[60] *De Memoria,* Chap. 1 (450*b* 1-13).

tion; and as to the question of which of the faculties within us memory is a function (it has been shown) that it is a function of the primary faculty of sense perception, i.e., of that faculty whereby we perceive time."[61]

Recollection—Its Laws.[62] "Recollection is neither the recovery nor the acquisition of memory; since at the instant when one at first learns or experiences, he does not thereby recover a memory, for none has previously existed; nor does he acquire it initially then. But when a disposition, as well as the experience, has once been produced, then memory is found; hence it does not come into being in conjunction with the origination of the experience in us.

"Further, when memory first has been produced in the individual and ultimate organ of sensation, the experience and the knowledge in question have already existed in the experiencing subject. But memory in the proper sense will not exist till after the lapse of time. We remember in present time what we have previously seen or heard; we do not now remember what we have now experienced."[63]

"But further, clearly, we may remember, not in virtue of a present act of recollection, but by being conscious of feeling the experience from the start. On the other hand, when we re-acquire the knowledge or perception or whatever it was, the permanence of which we called memory, here and now we have recollection of any of these. As a result we remember them and memory ensues; not that that can be said without restriction in all cases when previous experiences are repeated in consciousness; in some cases it is so but in others not, for the same man may learn or discover the same thing twice. Recollection then must differ from the latter operation; it requires a more considerable basis to start from than in the case of learning."[64]

The Law of Contiguity. "The occurrence of an act of recollection is due to the natural tendency of one particular change to follow another. If the sequence is necessary, it is clear that on the former change occurring, the second will be summoned into ac-

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[61] *De Memoria,* Chap. 1 (451*a* 15-19); see also Lewes (21, pp. 256-257); Brett (6, Vol. 1, p. 125); Pillsbury (25, p. 30); *De Anima,* pp. 54 ff.

[62] "The laws of Association here formulated by Aristotle are obviously merely principles governing the reinstatement of ideas previously experienced. Hence their scope is much narrower than that assigned to them by modern psychology" (27, p. 39). See also Woodworth (36, 1st ed., pp. 394-395).

[63] *De Memoria,* Chap. 2 (451*a* 22-34).

[64] *De Memoria,* Chap. 2 (451*b* 1-11).

tivity; when, however, the connection is not necessary but due to custom, the occurrence of the second process will take place only in most cases. It so happens that some people receive a greater bent from a single experience than others in whom the sequence has frequently taken place, and hence, in some instances, after seeing the things once we remember them better than others who have seen them frequently. Thus, when we recollect, one of our previous psychic changes is stimulated which leads to the stimulation of that one, after which the experience to be recollected is wont to occur. Consequently, we hunt for the next in the series, starting our train of thought from what is now present or from something else, and from something similar or contrary or contiguous to it. This is the means of effecting recollection; the change in those cases is now identical, now concomitant with, and now partially inclusive of the idea to be recalled, and hence the remainder formerly occurring subsequently to the rest is but small. This is the way in which the search for the idea not present is carried out, and even when there is no search, it is in this way that recollection occurs, where the one process occurs after the other; and in general it is after experience of other changes such as we have described that the process in question occurs. . . . "[65] "When one wishes to recall something, this is what he does—he tries to find the starting point of a process after which the one in question will recur. . . " "Those things are easily recalled which have an orderly arrangement such as we find in mathematics; but things wanting in exactitude are with difficulty remembered. . . . " "Recollecting differs also in this respect from relearning, that one who recollects will be able, somehow, to move, solely by his own effort, to the term next after the starting point. When one cannot do this of himself, but only by external assistance, he no longer remembers (i.e., he has totally forgotten, and therefore, of course, cannot recollect). Often when as yet unable to recollect, by searching one manages to do so and finds what he was seeking. This he succeeds in doing by setting up many movements until finally he excites one of a kind which will have for its sequel the fact he wishes to recollect. . . . "[66]

The Law of Similarity. "One must get hold of a starting point. This explains why it is that persons are supposed to recollect

[65] *De Memoria,* Chap. 2 (451*b* 1-29).
[66] *De Memoria,* Chap. 2 (451*b* 30-35, 452*a* 1-10).

sometimes by starting from mnemonic loci. The cause is that they pass swiftly in thought from one point to another, e.g., from milk to white, from white to mist, and thence to moist, from which one remembers Autumn, if this be the season he is trying to recollect. . . . "[67]

"The reason why, though the same link is employed, recollection sometimes is and sometimes is not successful, is that we can pass to a further distance at one time than at another. If the transition is mediated by some connecting link which has not lately been employed, one passes to the more familiar consequent, for the newly acquired habit has become exactly like a natural disposition. It is thus that we explain why frequently we recollect quickly what we have been meditating upon. It is just in accordance with a natural tendency to follow one another in particular order that things actually happen; and it is frequent repetition that produces a natural tendency."[68] "When we have to remember a name, we may recollect one like it and commit a verbal blunder as regards the proper one. This, then, is the way in which recollection takes place."[69]

The Law of Contrast. "The most important fact to be noticed for the purpose of recollection is that we should cognize determinantly or indeterminantly the time-relation of that which we wish to recollect. There is—let it be taken as a fact—something by which we distinguish a greater and a smaller time; and it is reasonable to think that we do this in a way analogous to that in which we discern spatial magnitudes; we know things that have great size and are at a distance, not by our thought reaching out to them there, as some say our sight does, but by a psychic process analogous to them. . . . . There exist in the mind figures and changes similar to the external objects. In what, then, is the difference between knowing the objects of greater size and knowing the other set which are smaller? In nothing, because all the internal (though smaller) are as it were proportional to the external. Now as we may assume within a person something proportional to the forms (of distant magnitudes), so, too, we may doubtless assume something else proportional to their distances. . . ."[70] "The time-apprehending process is twofold; sometimes one does not remember the internal with exact

[67] *De Memoria,* Chap. 2 (452*a* 13-17).
[68] *De Memoria,* Chap. 2 (452*a* 25-32).
[69] *De Memoria,* Chap. 2 (452*b* 5-7).
[70] *De Memoria,* Chap. 2 (452*b* 8-15).

precision as, e.g., that someone did something the day before yesterday, while in other cases his sense of time is accurate."[71]

"It has been stated already that those who have a good memory are not identical with those who are quick at recollecting. But the act of recollecting differs from that of remembering not only chronologically, but also in this, that many of the other animals have memory, but, of all that we are acquainted with, none, we venture to say, except man, shares in the faculty of recollection. The cause of this is that recollection is, as it were, a mode of inference. For he who endeavors to recollect, infers that he formerly saw or heard, or had some such experience; and the process by which he succeeds in recollecting is, as it were, a sort of investigation. But to investigate in this way belongs naturally to those animals alone which are also endowed with the faculty of deliberation; for deliberation is a form of inference."[72]

Imagination is neither sensation nor opinion, yet it is never called up without sensation. It is the faculty by which an image of some kind is called up within us, and is to be ranked with those faculties, such as sensation, opinion, and knowledge, by which we form judgments. The main function of imagination, apart from the interpreting of present sensations, is the formation of after-images of which Aristotle notes both the positive and the negative.[73]

No Thought Without Image. "We cannot think without imagery, for the same phenomenon occurs in thinking as is found in the construction of geometrical figures. There, though we do not employ as a supplementary requirement of our proof a determinateness in the size of the triangle, yet when we draw it we make it of a determinate size. Similarly in thinking also, though we do not think of the size, yet we present the object visually to ourselves as a quantum, though we do not think of its quantum. . . ."[74]

Reason. Aristotle distinguishes two kinds of reason, namely the active and the passive. The one is analogous to matter because it becomes all things; the other is analogous to the efficient cause be-

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[71] *De Memoria,* Chap. 2 (453*a* 1-3).

[72] *De Memoria,* Chap. 2 (453*a* 5-15); see also Brett (6, Vol. 1, p. 125); Pillsbury (15, p. 30); *De Anima,* Book 1, Chap. 4.

[73] *De Anima,* Book 2; Lewes (21, p. 240); Ross (29, pp. 142-143); *De Somnis,* Chap. 2 (459*b* 5).

[74] *De Memoria,* Chap. 1 (450*a* 1-8).

cause it makes all things. The one is an actuality, the other a potentiality.[75]

Active reason is not a reason which creates out of nothing. It works on a material given to it which it promotes from potentiality into actuality. It is an eternal activity, incorporeal, simple, passionless and stimulating. Its rôle is to make passive reason become its objects by apprehending them. It takes no impress from the circumstances of life; its knowledge has no marks of date or circumstances.[76]

Passive reason is a sort of plastic material on which the active reason impresses the forms of knowable objects. It is, as it were, the seat of ideas in man. It takes the impresses of circumstances and perishes at the death of its possessor. It knows only potentially.[77]

Opinion and Belief are mental states which combine the presentation of an image as representing an object with definite attitude toward that object, a connection with respect to it.[78]

Conviction is the work of reason and is not identical with pure imagination, for imagination as such does not involve reason. Opinion, belief, and conviction are parallel with imagination but not identical with it.[79]

Conation is that impulse or striving, within a human being, which reaches out after an object. When the object is given by the senses, the act is one of desire; when the reason exerts control the act is voluntary.[80]

Desire includes all activities that either secure pleasure or avoid pain; it is the principle of action in relation to pleasure and pain.[81]

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[75]Zeller (37, Vol. 2, pp. 97 ff.); Ross (29, pp. 148-153); Hicks (16, Book 3, Chaps. 4-8 and pp. 58-69); Brett (6, Vol. 1, pp. 134-136); Pillsbury (25, p. 32). The relation of the two intellects, as conceived by Wallace, is as follows: 'The creative reason is the faculty which constantly interprets and as it were keeps up the intelligible world for experience to operate upon, while the receptive reason is the intellect applying itself in all the various processes which fill our minds with materials of knowledge . . . the two it must be remembered are not two reasons; they are merely different modes of viewing the work of reason" (*De Anima,* pp. 98 and 115).

[76]Zeller (37, Vol. 2, pp. 97 ff.); Ross (29, pp. 148-153); *De Anima,* Book 2, Chaps. 4-8.

[77]Zeller (37, Vol. 2, pp. 97 ff.); see also *Politics,* Book 7, Chap. 14.

[78]Brett (6, Vol. 1, p. 133); *Nichomachean Ethics,* Book 3, Chap. 4.

[79]Brett (6, Vol. 1, p. 137); *De Anima,* Book 1, Chap. 9; Book 2, Chap. 2; Zeller (37, Book 3, Chaps. 9-12; Book 2, Chap. 7); *Nichomachean Ethics,* Book 6, Chap. 2.

[80]*Nichomachean Ethics,* Book 10, Chap. 2.

[81]Welldon's (34) *Rhetoric,* Book 2, Chap. 1; *Nichomachean Ethics,* Book 6, Chap. 2; *De Anima,* Book 7, Chap. 9; Book 2, Chap. 2.

Emotions. Aristotle discusses the emotions fully and carefully in his *Rhetoric.* They are discussed there because they are one of the means of persuasion that should be used by the speaker to move his audience. By emotion he means all such states as are attended by pain and pleasure and produce a change or difference in our attitude as judges, e.g., anger, compassion, fear, and the like, and their opposites.[82] There are fourteen emotions in all.[83] Each is treated under three heads:

1. The conditions under which people are liable to it,
2. Those persons who are usually the objects of it,
3. The causes of it.

1. "*Anger* is an impulse, attended with pain, to an unconcealed revenge on account of a conspicuous slight shown in some offense against oneself or one of one's friends without any natural reasons for the slight. The conditions under which one becomes angry are annoyance, disappointment, unsatisfied desire, etc. The objects of anger are the persons who sneer at us, who disparage our accomplishments, who requite our services with ingratitude, etc. The objects and conditions of anger may be thought of as its causes."[84]

2. "*Placability* may be defined as a settling down or quieting of anger. The conditions which bring about placability are amusement, feasting, prosperity, etc. Those who do not slight us or who slight involuntarily or who, if they offend against us, confess their fault, and are sorry for it, are the persons towards whom we are placable. The causes of placability are the opposites of anger."[85]

3. *Love.* "To love a person is to wish him all such things as you suppose to be good, not for your own sake but for his, and to be ready so far as in you lies to effect them. The conditions of love are altruistic tendencies. The objects of love are benefactors, liberal, brave and just people. The causes of love are favors conferred, especially if conferred spontaneously and without ostentation."[86]

4. "*Hatred* is the opposite of love and the causes which produce it are anger, spite and prejudices. It differs from anger in this respect. Anger arises from personal wrongs, hatred is independent of personal wrongs. The former is directed against individuals, the

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[82] Welldon's (34) *Rhetoric,* Book 2, Chap. 1.
[83] Compare these with McDougall's (22*a*) principal instincts and the primary emotions of man.
[84] *Rhetoric,* Chap. 2; *Nichomachean Ethics,* Book 3, Chap. 9.
[85] *Rhetoric,* Chap. 3.
[86] *Rhetoric,* Chap. 4.

latter against classes. Anger is curable by time but hatred is not. The first aims at causing pain and the second evil. The one is accompanied by pain and the other is not. Anger admits of compassion while hatred precludes it."[87]

5. *"Fear* is a species of pain or disturbance arising from an impression of evil which is destructive or painful in its nature. The condition of fear is one's own feeling of being capable of suffering and suffering at the hands of particular people. The persons who appear to have a great power of destroying or inflicting such injury as tends to grievous pain are the objects that inspire fear."[88]

6. *Confidence* is hope accompanied by an impression of salutary things as near at hand and of formidable things non-existent or remote. The conditions of confidence are frequent successes in the past, frequent escapes from danger, the consciousness of innocence, etc. The sources of confidence are the remoteness of danger and the possession of remedies or means of rectification."[89]

7. *"Shame* is a species of pain or disturbance in regard to evil things, either past, present or future which have an appearance of tending to ignominy. Conditions of shame are brought about when we are conscious of past achievements or actions upon which we shall bring dishonor, or when we are likely to live in the presence of the witnesses of our shame. The persons who inspire a feeling of shame are those who admire us, or whom we admire, or whose admiration we desire to win. The cause of shame is the individual's awareness of his shameful performance, such as throwing away one's shield in battle, refusing to restore a deposit, etc."[90]

8. *"Shamelessness* is a species of slight or indifference in regard to the same things that have been enumerated in the definition of shame. Topics opposite to those of shame will explain shamelessness."[91]

9. *Benevolence.* "It is benevolence in virtue of which the person in whom it resides is said to render a service to anybody in the hour of need, not in return for previous services or for any personal benefit to him who renders it, but for the benefit of the recipient alone. An act of benevolence may be enhanced by showing that it

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[87] *Rhetoric,* Chap. 4.
[88] *Rhetoric,* Chap. 5; *Nichomachean Ethics,* Book 3, Chap. 10.
[89] *Rhetoric,* Chap. 5.
[90] *Rhetoric,* Chap. 6; *Nichomachean Ethics,* Book 4, Chap. 15.
[91] *Rhetoric,* Chap. 6.

was done at a critical moment or that it has never been done before."[92]

10. "*Compassion* is a sort of pain at an evident evil of destructive or painful kind, in the case of somebody who does not deserve it, the evil being one to which we may naturally expect ourselves or some one of our friends to be liable, and this at a time when it appears to be near at hand. The condition of compassion is a capacity for similar suffering. Its objects are familiar friends, equals in age and character, etc. The causes of compassion are all such painful and distressing things as are destructive and ruinous, and all the evils of fortune."[93]

11. "*Virtuous indignation* is a feeling of pain at such prosperity as is apparently unmerited. Conditions of virtuous indignation come to be whenever we consider ourselves to be worthy of particular things, and others to be unworthy of them. Persons who are the object of virtuous indignation are those who acquired wealth unexpectedly and those who received promotion in competition with their superiors. Not all good things, such as justice or valor, may be considered as causes of virtuous indignation, but only such good things, when possessed by the bad, as seem to be rightly the possessession of the good."[94]

12. "*Envy* is a species of pain felt at conspicuous prosperity on the part of persons like ourselves in respect of certain goods and thus not with any view to our personal advantage but solely because they are prosperous. Both virtuous indignation and envy are painful and perturbing emotions. Each relates to the prosperity of others. The former, however, relates to the prosperity of some one who is undeserving, while the latter to that of some one like ourselves. The conditions that bring about envy are one's awareness of others like him in race, family, age, habit of mind, reputation and possessions, ambition and mean-mindedness. The objects of envy are one's equals and rivals, while its causes are achievements or possessions of which one covets the reputation, and the gifts of fortune."[95]

13. "*Emulation* is a species of pain at the manifest presence of such goods as are highly valued and also attainable by ourselves

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[92] *Rhetoric*, Chap. 7.
[93] *Rhetoric*, Chap. 8.
[94] *Rhetoric*, Chap. 9.
[95] *Rhetoric*, Chap. 10.

in persons who have a natural resemblance to us, and this not because somebody else is in possession of them, but because we are not equally in possession of them ourselves. The condition that brings about emulation is one's consideration of himself as entitled to goods which he does not enjoy. The objects of emulation are persons who possess such goods as are held in honor, e.g., virtues. The causes of emulation are the virtues and whatever other goods are held in honor."[96]

14. "*Contempt* is the opposite of emulation, and topics opposite those of emulation will explain contempt."[97]

The speaker who can best appeal to his audience's emotions is the speaker who will be most successful in his undertakings.

Katharsis. Perhaps no other word in ancient literature has been more frequently handled by commentators, critics, and poets, by men who knew Greek and by men who knew no Greek, than the word *katharsis,* which Aristotle used in his *Poetics,* defining tragedy, and in his *Politics* in connection with the effect which the various melodies have on the soul of people easily aroused to enthusiasm. It would be an interesting study to collect the many and strange translations and interpretations that have been given to these two definitions in the last four hundred years. But that is another study and a treatise beyond the scope of this article.[98] The meaning of katharsis, as will be seen in the quotations following, is sufficiently clear. And the student of psychology should not be satisfied with the interpretation of any other man save Aristotle. "Tragedy is a representation of an action that is heroic and complete and of a certain magnitude, by means of language enriched with all kinds of ornament, each used separately in the different parts of the play; it represents men in action and does not use narrative, and through pity and fear it effects relief (katharsis) to these and similar emotions. . . ."[99] "In the case of the sacred melodies we observe that such persons, after listening to melodies which raise the soul to ecstacy, relapse into their normal condition as if they had experienced a medical or purgative treatment (katharsis). The same is, of course,

[96] *Rhetoric,* Chap. 11.
[97] *Rhetoric,* Chap. 11.
[98] For an account of numerous interpretations of katharsis read: Zeller (37, Vol. 2, Chap. 15); Ross (29, Chap. 9); Butcher (9*a*, Chap. 6).
[99] *Poetics,* Book 6, Chap. 2.

the case with compassionate and fearful persons and emotional persons generally, and with others in proportion as each participates in such emotions; they all experience a sort of purging and a pleasureable feeling of relief. Similarly melodies of practical sort produce in men a feeling of innocent joy."[100]

Pleasure. Aristotle (unlike Plato, who held that all pleasure is merely a transition, a passage from pain, and consequently of a negative or relative character) teaches that there are perfect pleasures. "Pleasure," he argues, "is not a process of motion or production, as it is not characterized by quickness and slowness. It is not a satisfaction of the natural state of man's being, although the process of satisfaction may be attended with pleasure. It is the pleasures of eating and drinking which have given rise to the theory that pleasure is a process of satisfaction; but other pleasures, e.g., the pleasures of mathematics, have no antecedent pain. There are immoral as well as moral pleasures, but they do not prove pleasure to be a bad thing, as they are not pleasant except to people who are in a bad condition. The truth seems to be this: Pleasure is not the good, nor are all pleasures desirable, but only such pleasures as are not immoral in their origin."[101]

"Pleasure, like sight, is perfect at any time; it is a whole; it is not divisible into parts; it is not a motion or process of production, and therefore duration of time does not make it more perfect."[102] "Pleasure is most perfect and most pleasant when the particular sense which exercises its activity upon its own object is in itself in a sound condition, and the object that falls within its domain is the noblest."

The incapacity of human nature to be continuously active renders the feeling of continuous pleasure impossible. "Pleasure, however, perfects the activities; and therefore perfects life, which is the aim of human desire."[103]

There are different kinds of pleasures, e.g., those of the intellect and those of the senses. They differ in goodness or badness in the same manner as all activities. The pleasures of unlike beings differ.

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[100] *Politics,* Book 5, Chap. 7.
[101] *Nichomachean Ethics,* Book 10, Chap. 2.
[102] *Nichomachean Ethics,* Book 10, Chap. 3.
[103] *Nichomachean Ethics,* Book 10, Chap. 4.

Beings of like kind have different pleasures. Hence what is pleasant to one man is unpleasant to another. The true pleasures are the pleasure of the virtuous man, and the pleasures that belong to the activity of the perfect man are the perfect pleasures.[104]

Courage is a mean state in regard to the causes of confidence and fear—it is the consciousness of danger accompanied by assurance of successful resistance. It chooses action, it endures pain, from love of nobleness or fear of disgrace.[105]

Joy and Pity come under the general head of wish, and are attitudes of mind accompanied by imagination of good or evil whether for oneself or for others.[106]

Will for Aristotle is ultimately reason; it is conation completely rationalized.[107]

Knowledge is grounded on sensuous perception, but this is the foundation for higher knowledge, of which intellect only is cognizable. But this does not exist in and by itself; without sensation no one could know anything.[108] All concepts are derived from and accompanied by definite images, without which the soul does not think.[109] Knowledge in activity is identical with the object; but in potentiality it pre-exists in the individual. Yet rigorously speaking, that cannot be said to pre-exist which sometimes is, and sometimes is not, reflected on. But that alone, whatever it be, which is separate from anything else, is deathless and eternal. We have no memory of it because it is passionless; and the impressible mind is perishable and without it there can be no reflection.[110]

Sleep is an affection of the general faculty which does not preclude such exercise of this faculty as takes place in dreaming.[111] "It is a

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[104]*Nichomachean Ethics,* Book 10, Chap. 5.
[105]*Nichomachean Ethics,* Book 3, Chaps. 9-12; Book 2, Chap. 7.
[106]Brett (6, Vol. 1, pp. 139-140); *Nichomachean Ethics,* Book 10, Chap. 2.
[107]Brett (6, Vol. 1, pp. 11-12); Zeller (37, Vol. 2, pp. 108 ff.).
[108]*De Anima,* Book 3, Chap. 8; Pillsbury (25, p. 33).
[109]*De Anima,* Book 3, Chap. 7.
[110]Lewes (21, p. 241); *De Anima,* Book 3, Chap. 5.
[111]Beare's *De Somno et Vigilia,* Chap. 2 (455*a* 1-12, note, a25-7); Lewes (21, pp. 260-265).

sort of concentration or natural recoil of the hot matter inward."[112] "It comes on when the corporeal element in the evaporation is conveyed upward by the heat, along the veins, to the head."[113] "We have stated the cause of sleeping, viz., that it consists in the recoil by the corporeal element, upborne by the connatural heat, in a mass upon the primary sense-organ. We have also stated what sleep is, having shown that it is a seizure of the primary sense-organ, rendering it unable to actualize its powers; arising of necessity (for it is impossible for an animal to exist if the condition which render it an animal be not fulfilled), i.e., for the sake of its conservation; since remission of movement tends to the conservation of animals."[114]

"*Waking* is the opposite of sleep. . . ." "The criterion by which we know the waking person to be awake is identical with that by which we know the sleeper to be asleep; for we assume that one who is exercising sense-perception is awake, and that every one who is awake perceives either some external movement or else some movement in his own consciousness."[115] "The waking state is for an animal its highest end, since the exercise of sense-perception or of thought is the highest end for all beings to which either of these appertain; inasmuch as these are best, and the highest end is what is best."[116]

"A person awakes from sleep when digestion is completed, and the heart, which has been previously forced together in large quantity, within a small compass from out the surrounding part, has once more prevailed, and when a separation has been affected between the more corporeal and the purer blood. The finest and purest blood is that contained in the head, while the thickest and most turbid is that in the lower parts. The source of all the blood is the heart. Now of the chambers in the heart the central communicates with each of the two others. Each of the latter again acts as receiver from each respectively, of the two vessels,[117] called the 'great' and the 'aorta.' It is in the central chamber that the separation takes place. . . . ." "Owing to the fact that the blood

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[112] *De Somno et Vigilia,* Chap. 3 (457*b* 1-2).
[113] *De Somno et Vigilia,* Chap, 3, (457*b* 20-22).
[114] *De Somno et Vigilia,* Chap. 3 (458*a* 25-33).
[115] *De Somno et Vigilia,* Chap. 7 (454*a* 1-4).
[116] *De Somno et Vigilia,* Chap. 2 (455*b* 22-5).
[117] "To use the term 'artery' here in translation would mislead any mere English reader into thinking Aristotle knew the difference between arteries and veins" (Beare's *De Somno,* Chap. 3 [458*a* footnote]).

formed after the assimilation of food is especially in need of separation, sleep occurs and lasts until the purest part of this blood has been separated off into the upper parts of the body, and the most turbid into lower parts; when this has taken place animals awake from sleep, being released from the heaviness consequent on taking food."[118]

"*Dream*[119] is a sort of presentation, and, more particularly, one which occurs in sleep. Illusions, however are not dreams, nor is any other phantom a dream which presents itself when the sense-perceptions are in a state of freedom. Nor is every presentation which occurs in sleep necessarily a dream. For in the first place, some persons when asleep actually, in a certain way, perceive sounds, light, savour, and contact; feebly, however, and, as it were, remotely. For there have been cases in which persons while asleep, but with the eyes partly open, saw faintly in their sleep as they supposed the light of a lamp, and afterward, on being awakened, straightway recognized it as the actual light of a real lamp; while, in other cases, persons who faintly heard the crowing of cocks or the barking of dogs identified these clearly with the real sounds as soon as they awoke. Some persons, too, return answers to questions put to them in sleep. For it is quite possible that, waking or sleeping, while the one is present in the ordinary sense, the other also should be present in a certain way. But none of these occurrences should be called a dream. The dream proper is a presentation based on the movement of sense impressions, when such presentation occurs during sleep, taking sleep in the strict sense of the term."[120]

The Causes of Dreams. "The movements which occur within the body in the day time are lost sight of in contrast with the waking movements, which are more impressive. In sleep the opposite takes place, for then even trifling movements seem considerable. This is plain in what often happens during sleep; for example, dreamers fancy that they are affected by thunder and lightning, when in fact there are only faint ringings in their ears; . . . or that they are walking through fire, and feeling intense heat, when there is only a slight warmth affecting certain parts of the body. When

[118]*De Somno,* Chap. 3 (458*a* 10-24); Brett (6, Vol. 1, pp. 125-126); Lewes (21, pp. 258-260).

[119]The fact that Aristotle in his treatise on dreams does not refer to the opinions of any predecessor led some to infer that the dream theory is entirely his own. See Lewes (21, p. 266).

[120]Beare, *De Somnis,* Chap. 3 (462*a* 15-31); Lewes (21, pp. 264-266).

they are awakened, these things appear to them in this their true character. . . . It is not improbable that some of the presentations which come before the mind in sleep may even be causes of the actions cognate to each of them. For as when we are about to act in waking hours, or are engaged in any course of action, or have already performed certain actions, we often find ourselves concerned with these actions, or performing them in a vivid dream; the cause whereof is that the dream-movement has had a way paved for it from the original movement set up in the day time; exactly so, but conversely, it must happen that the movements set up first in sleep should also prove to be starting-points of action to be performed in the day time, since the recurrence by day, of the thought of these actions, also has had its way paved for it in the images before the mind at night. Thus then it is quite conceivable that some dreams may be tokens and causes of future events."[121]

The Prophetic Aspect of Dreams.[122] "As to the divination which takes place in sleep and is said to be based on dreams, we cannot lightly either dismiss it with contempt or give it implicit confidence. The fact that many persons suppose dreams to possess a special significance tends to inspire us with belief in such divination as founded on the testimony of experiences; and indeed that such divination in dreams should, as regards some subjects, be genuine, is not incredible, for it has a show of reason; for which one might form a like opinion also respecting all other dreams. Yet the fact of your seeing no probable cause to account for such divination tends to inspire us with distrust. For in addition to its further unreasonableness, it is absurd to combine the idea that the sender should be God with the fact that those to whom he sends them are not the best and wisest but merely commonplace persons,[123]—whose mind is not given to thinking, but, as it were, derelict or totally vacant, and when once set moving is borne passively on in the direction taken by that which moves it.[124] If, however, we abstract from the causality of God, none of the other causes assigned appears probable. For that certain persons should have foresight in dreams concerning things destined to take place at the Pillars of Hercules, or on the banks of the Borysthenes, seems to be something to discover, the explanation of which surpasses

[121]Beare, *De Somnis,* Chap. 3 (463*a* 21-30).
[122]For evaluation of his dream theory read Lewes (21, p. 263).
[123]Beare, *De Divinatione per Somnum,* Chap. 1 (462 1-22).
[124]Beare, *De Divinatione per Somnum,* Chap. 2 (464*a* 23-25).

the wit of man. Well, then, the dreams in question must be regarded either as causes or as tokens of the events, or else as coincidences; either as all, or some of these, or as one only."[125]

Prophetic Dreams Are Mere Coincidences. "Most of the so-called prophetic dreams, are, however, to be classed as mere coincidences, especially all such as are extravagant, and those in the fulfillment of which the dreamers have no initiative, such as in the case of a sea-fight, or of a thing taking place far away. As regards these it is natural that the fact should stand as it does whenever a person, on mentioning something, finds the very thing mentioned come to pass. Why, indeed, should this not happen also in sleep? The probability is, rather, that many such things should happen. As when one's mentioning a particular person is neither token nor cause of this person's presenting himself, so, in the parallel instance, the dream is to him who has seen it, neither token nor cause of its so-called fulfillment, but a mere coincidence. Hence the fact that many dreams have no 'fulfillment,' for coincidences do not occur according to any universal or general law. . . ."[126] "That certain persons in particular should have vivid dreams, e.g., that familiar friends should thus have foresight in a special degree respecting one another, is due to the fact that such friends are most solicitous on one another's behalf."[127]

These, in brief, are Aristotle's chief psychological arguments. His psychological treatises, it is true, do not equal those of modern authors either in organization or in thoroughness, but this should not be held against him, for he was a pioneer in this field. It is not difficult to imagine what his contributions to psychology would have been had he had at his disposal the accumulated experience of the men who have thought and written on this subject since his time and the conveniences enjoyed by modern psychologists. Even without these aids he made unequaled contributions which until recently were accepted as authoritative. Many of the theories which modern authors propound as new are his, slightly modified or more clearly stated. Through his works he has influenced the world's thought more than any other man that ever lived. While the writings of Aristotle are somewhat confusing, yet they are stimulating, and to the seekers for truth they will always be an ever-flowing source of inspiration.

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[125]Beare, *De Divinatione per Somnum,* Chap. 2 (463*a* 21-30).
[126]Beare, *De Divinatione per Somnum,* Chap. 2 (463*b* 1-10).
[127]Beare. *De Divinatione per Somnum,* Chap. 2 (464*a* 27-30); Lewes (21, p. 266); Pillsbury (25, p. 32).

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LA PSYCHOLOGIE D'ARISTOTE: UN ORDRE LOGIQUE DE SES *DE ANIMA* ET TRAITÉS DE PAREILLE NATURE

(En Form Synoptique)

(Résumé)

L'auteur a recueilli des morceaux de plusieurs des livres d'Aristote (*Métaphysique, Psychologie* (*De Anima*), *Le Sens et Ses Objets, Mémoire et Rappel, Le Sommeil, Les Rêves, La Divination dans les Rêves, la Morale, la Rhétorique, La Poétique,* et *la Politique*) dans le but de montrer les rapports des diverses contributions l'une avec l'autre et avec les postulats philosophiques basiques d'Aristote. "La matière et la forme, les deux principes constitutifs d'un objet quelconque," sont le sujet de la *Métaphysique,* et il faut comprendre la discussion de celles-ci par Aristote si l'on désire comprendre la *Psychologie.* Le premier livre de la *Psychologie* (*De Anima*) contient une revue historique de la psychologie avant Aristote; le deuxième une définition de l'âme et une discussion semi-inductive des sens et le troisième une explication des phénomènes mentaux. *Le Sens et ses Objets* considère spécialement les phénomènes de la vision, du goût, et l'odorat, et développe l'analyse des sens donnée dans la *Psychologie. Mémoire et Rappel* inclut une formulation briève des lois de l'association. Les traités sur le *Sommeil,* les *Rêves,* et la *Divination dans les Rêves* incluent une description naturaliste du symbolisme des rêves et des rêves "prophétiques." La dis-

cussion des trois qualités de l'âme (les émotions, les facultés, et les états moraux) se trouve dans la *Morale* et la *Rhétorique*. La *Poétique* et la *Politique* contiennent des descriptions de la "doctrine de katharsis."

LOUKAS

DIE PSYCHOLOGIE VON ARISTOTLES: EINE LOGISCHE ANORDNUNG SEINES *'DE ANIMA'* UND VERWANDTE ABHANDLUNGEN

IN SYNOPTISCHER FORM

(Referat)

Der Verfasser hat Stoff zusammengebracht aus vielen von Aristoteles' Schriften (*Metaphysik, Psychologie* (*De Anima*), *Die Sinnesempfindung und ihre Gegenstände, Gedächtnis und Erinnerung, Der Schlaf, Träume, Vorahnung in Träumen* (Divination in Dreams), *Ethik, Rhetorik, Poetik,* und *Politik*) in der Absicht, die inneren Zusammenhänge zu erweisen zwischen den verschiedenen Schriften und mit Aristoteles' philosophischen Grundpostulaten. "Stoff und Form, die zwei Grundbestandteile jedes Gegenstandes," bilden das Thema der *Metaphysik,* und Aristoteles' Besprechung dieser Grundprinzipien muss zuerst verstanden sein, soll die *Psychologie* verständlich werden. Das erste Heft der *Psychologie* (*De Anima*) enthält einen historischen Rückblick über die vor-Aristotelische Psychologie, das zweite eine Erörterung der psychischen Erscheinungen. *Die Sinnesempfindung und ihre Gegenstände* behandelt besonders die Erscheinungen des Sehens des Geschmacks, und des Geruchs, und erweitert die in der *Psychologie* gegebene Analyse der Sinnesempfindung. *Gedächtnis und Erinnerung* enthält u. a. auch eine kurze Formulierung der Gesetze der assoziativen Verbindung. Die Abhandlungen über den *Schlaf, Träume,* und *Vorahnung in Träumen* schliessen einen naturalistischen Bericht über die Traumsymbolik und über "prophetische" Träume in sich ein.

Die drei Qualitäten der Seele (Gemütsbewegungen, Fähigkeiten, und moralische Zustände) werden in der *Ethik* und in der *Rhetorik* behandelt die *Poetik* und die *Politik* erstatten über die "Lehre der Katharse" (Katharsis) Bericht.

LOUKAS

SHORT ARTICLES AND NOTES

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ERRATUM

In the article on "Pain Adaptation: A contribution to the von-Frey–Goldscheider Controversy," by L. B. Hoisington and E. F. Wells (*J. Gen. Psychol.*, 1931, **5**, 352-367) Footnote 16, page 365 should read: "Von Frey has already admitted the possibility of pressury [*not* pressure] adaptation under certain conditions (10, p. 243; 11, p. 328)."

THE EFFECT OF CIGARETTE SMOKING ON SECRETION[1]

A. L. Winsor

In both physiological and psychological studies of the effect of tobacco smoking on organic functions, where smokers and non-smokers have been used as subjects, significant differences have been reported between the reactions of the two groups. A smoke may have an accelerating influence on certain types of behavior with the non-smoker, while it has a retarding effect on the same type of behavior with the smoker. The usual conclusion has been that the effect of tobacco on the novice could not be reliably compared with its effect upon the habituated smoker. Here investigation seems to have stopped, although the logic of the situation suggests the need for a further study of the organic nature of the process of habituation which makes it possible for these opposing results to be obtained.

At the present time no one seems to have determined what aspect of behavior is primarily involved in the habituation process. It is not known whether or not it is fundamentally a process of motor, mental, or glandular adjustment. It is not known whether or not it is a change from excitation to inhibition or from inhibition to excitation. Although most of the studies have concerned themselves with variations in motor or mental activity, it

[1]The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research established by August Heckscher at Cornell University.

occurred to us that more profound changes might be found in an analysis of secretory function. Of all organic systems the digestive is most directly concerned with the induction of substances in the oral cavity and must bear the burden of the adjustment. Therefore, any technique which would register variation in the processes of digestion might yield some suggestion relative to the changes which characterize the behavior of the smoker and the non-smoker and lead to an explanation of the habituation process. Furthermore, the fact that the viscera are assumed to be intimately connected with the affective states, and that tobacco smoking is concerned with these changes provides a further reason to expect a study of glandular activity in connection with smoking to be fruitful. Consequently, we arranged a study that would permit a quantitative analysis of secretory changes during and subsequent to cigarette smoking. Both smokers and non-smokers were used as subjects.

Since a technique for collecting and measuring parotid secretion was already arranged for in the laboratory, it was decided to use this gland for the study. Our method as well as the nature of the continuous flow of saliva under normal conditions has been considered in previous reports (5, 4) and need not be repeated. It should be observed, however, that in these previous studies parotid inhibition, under indirect stimulation, was found to coincide with the functioning of the sympathetic division of the autonomic system. The changes observed in this study which are due to indirect stimulation may be assumed, therefore, to be representative of sympathetic functioning.

The procedure adopted was simple. After a suction disc had been securely fastened over the Stenson's duct and the disc connected with the measuring apparatus, the subject was instructed to maintain a comfortable sitting-posture and to avoid tongue and throat movements. He was not allowed to speak during the experiment and swallowed on signal once every five minutes. The rate of secretion was recorded at the end of every minute in terms of hundredths of a cubic centimeter. Five adults who did not smoke and five who smoked regularly were used as subjects.

As soon as the increased flow of saliva due to recent physical activity had subsided, the experiment began. A control period of 20 minutes was allowed as a basis of comparison. At the end of this period a cigarette was placed in the subject's mouth and lighted.[2] He was instructed to smoke it for five minutes. Inasmuch as the smokers inhaled part of the smoke, the non-smokers were requested to do the same. At the end of the five-minute period, the stub of the cigarette was thrown away and the subject cleaned out his mouth and resumed the control posture. The secretion for the suc-

[2]Incidentally, it is of interest to note that the habitual user of tobacco shows a conditioned salivary response to the sight of cigarettes under favorable conditions.

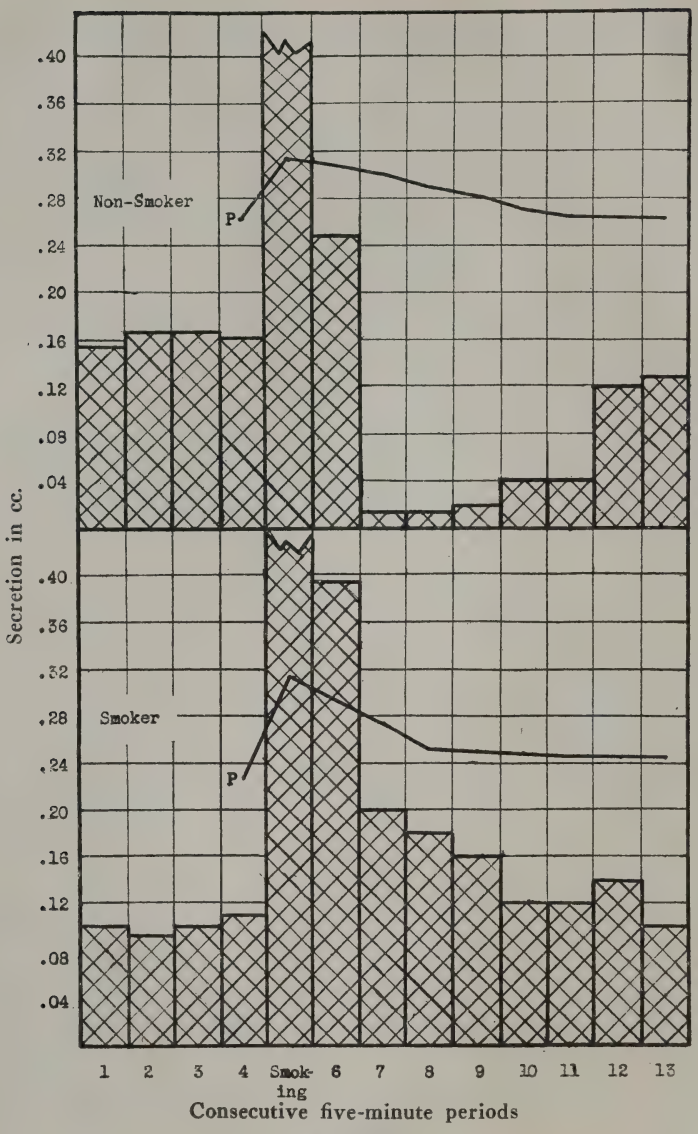

FIGURE 1

EFFECT OF SMOKING ON PAROTID SECRETION OF SMOKER AND NON-SMOKER
P=pulse-rate during twenty-second intervals

ceeding 30 or 40 minutes was carefully recorded. In order that variation in heart action might be compared with secretory changes, a pulse reading was taken every five minutes.

Instead of preparing tables of average performance, it was considered more significant to present in graphical form typical records of smokers and non-smokers for comparison. The graphs in Figure 1 show the variation in the rate of secretion during the experiment. For convenience in presentation, the minute periods have been consolidated into five-minute intervals. Thus, the first four intervals in both graphs represent the twenty-minute control period. Column 5 represents the smoking period, but space would not permit indication of the entire volume of secretion for this interval which was .87 cc. in the upper and .84 cc. in the lower record. Our concern was not with the rate of secretion during the smoking period but with the subsequent changes. In the case of the non-smoker, it will be observed that the smoking period was followed by a reduction in secretion from the excessive amount secreted during the smoking process, and that the next intervals (Columns 7, 8, 9) show almost total inhibition of secretion followed by a gradual return to normal.

Up to and including Column 5 the record for the smoker shows the same variations as noted in the record of the non-smoker. It is during the periods following the smoking that significant differences appear. In general, smoking is followed by prolonged excitation in the case of the smoker as against inhibition for the non-smoker. The duration of the inhibition for the non-smoker is about the same as the duration of excitation for the smoker. Although there were marked individual differences among the different subjects to tolerate this drug, the initial excitation during smoking was always followed by some degree of inhibition for the non-smokers. It is this tendency for inhibition as an initial experience and excitation as an end result that invites consideration.

Evidence that the inhibitory effect noted above extends to other parts of the sympathetic system is suggested by Carlson's (2) observations of the young man (a non-smoker) with a permanent gastric fistula. Whenever this subject smoked a cigar, inhibition of the hunger contractions resulted. It seems logical to assume, therefore, that the inhibition of oral secretion observed in our experiment is representative of the sympathetic system as a whole, as shown in an earlier report. This being the case, behavior during this inhibitory period would show the characteristics of sympathetic innervation. As Cannon (1) and others have shown, sympathetic innervation operating during an emotional state represents emergency adjustment. Important changes in the blood stream result from the reciprocal innervation, and the organism is capable of increased speed, greater strength, and a new resistance to fatigue. If it could be shown that studies of the behavior of non-smokers indicate these general characteristics of sympathetic innerva-

tion when they smoke, there would be further evidence for the data presented above.

An examination of the extensive study of the effect of tobacco on the behavior of smokers and non-smokers made by Hull (3) throws some light on this point. In summarizing the effects of habituation on behavior, this author makes the following statement: "Certain processes (resistance to fatigue, accuracy in addition, memory span) do show a diminution in effect as the result of habituation." In other words, these aspects of behavior, all of which are capable of relatively accurate measurement, showed a better record for the non-smoker than the habituated smoker following a period of smoking. Thus we have substantial evidence on the behavior side that inhibition of secretion or sympathetic functioning characterizes the novice in his early experience with tobacco.

Turning to affective behavior for support of our data, there is little in the way of concrete evidence on which we may draw. However, it is generally recognized that tobacco smoking is not a pleasant experience for the uninitiated but that, with habituation or the development of gastric tolerance, smoking is accompanied and followed by a feeling of satisfaction or pleasure. Many psychologists now support the theory that sympathetic innervation causes or is characterized by an unpleasant affective state, while cranial functioning causes or is characterized by a pleasant affective state. This theory is in accord with our explanation of tobacco effects.

It is frequently stated that strong emotions act much like drugs. Here is evidence that drugs act like emotions. The first effect is inhibitory but continued use causes the inhibition to break down. It appears likely that the habituation process in this case is a reduction of inhibitory innervation. Consideration of the many factors which influence the rate and extent of this reduction must await further research.

The curves (*P*) shown in Figure 1 indicate variation in pulse-rate during the experiments. The points on the curve represent the rate for twenty-second intervals taken every five minutes. The fact that tobacco increased heart action for the smoker as well as the non-smoker is in keeping with known facts. Hull found that "habituation to the use of tobacco seems to have little or no tendency to establish a tolerance with respect to heart rate (3, p. 53).

Summary

In a study of the effect of cigarette smoking on the parotid secretion of both smokers and non-smokers, it was found that smoking is followed by inhibition of secretion in the case of the non-smoker, and by excitation of secretion in the case of the smoker. This was interpreted as evidence that habituation to the effects of tobacco involves a reduction of the initial sympathetic innervation. Conclusions from studies of the effect of tobacco on behavior were cited which indicate that the non-smoker shows evidences in his behavior of sympathetic innervation after smoking.

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NITROUS OXIDE ANESTHESIA AS AN EXPERIMENTAL TECHNIQUE IN PSYCHOLOGY[1]

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Following a suggestion of Dr. A. G. Bills, the author carried out a series of preliminary investigations to determine the applicability of nitrous oxide anesthesia as a technique in the study of central dissociation and other problems in psychology. The following facts came out early in the experimentation:

1. *Under certain concentrations subjects pass through several stages slowly before becoming unconscious.* For concentrations of NO_2 above 40% (given with O_2), the subject passes through several stages of responsiveness. These are of interest to the psychologist. This technique might be used in problems involving the question of the stability of different forms of reaction as determined by their order of disappearance. Reflexes, verbal habits, and motor habits might be compared; also the disappearance of sensations and the operation of habits on different levels of alertness could be investigated.

2. *It seems applicable to psychological problems.* In addition to the problems stated above, the relation of partial anesthesia to the technique of hypnosis needs study. It may be that this technique can be used in preference to hypnosis in being more objective, more easily controlled, and universally more applicable. Surely this problem is worthy of some investigation. The relation of anesthesia to other phenomena of an abnormal nature in psychology, as dissociation, needs study.

3. *Subjects can be kept unconscious for a definite lapse of time.* This technique may be used in experimental studies of dreams, in studies of the effect of no retroaction on memory material, and in studies in the estimation of time.

[1]Anesthesia administered by Dr. H. M. Livingstone, working under a grant from the Douglas Smith Foundation for Medical Research of the University of Chicago.

4. *Suggestion can be controlled.* Naïve subjects have difficulty in knowing whether or not they are receiving the gas. In experiments of this sort it is imperative to control suggestion, as such a variable can obscure results.

5. *It can be given in low concentrations, permitting the subject to respond.* In this experiment concentrations of NO_2 as low as 25% were used, together with O_2. This gave a partial anesthesia and the subjects remained responsive for 40 minutes and showed indications that they could remain so for quite a time longer. It seems that, with concentrations below 70%, the subjects can remain responsive for 10 minutes or more, depending upon the individual and the concentration.

6. *It is easily administered.* Since the experimenter must be free to give tests, it is best to get an interne or an advanced medical student with experience in giving anesthesia. The subject should be examined briefly, as the gas has contra-indications which are modified with skilled administration. However, the gas ranks above ether and chloroform as far as safety to the life of the subject is concerned. No special rooms or apparatus are necessary except a small portable administering apparatus. The kind used by many dentists may be employed.

7. *It requires little preparation on the part of the subject.* The subject may come in from his daily routine, be seated, and take the gas without change of attire or other preparation. It is well, however, to advise him not to eat anything for a few hours previous to the experiment.

8. *It has practically no after-effects.* After a half-hour or more of experimentation, the subject may leave, feeling practically no ill-effects. ". . . It is practically free from sequelae if administered with a fair degree of care" (2).

In spite of these above advantages, perusal of the literature revealed no controlled experiments save one (1) in which an M.D. was used as the only subject without the control of suggestion. Several early papers gave descriptions of consciousness under anesthetics (4). A review of the literature on anesthesia showed only qualitative statements, apparently gathered by physicians, as to the effect of the gas on mental processes.

Therefore, before any psychological use could be made of this technique, exploratory work had to be performed in order to ascertain its possibilities for psychology. It was necessary to learn which sensory and motor avenues were open under different concentrations, and how different psychological processes functioned under the gas. For this reason, laboratory tests were used covering a large range of functions. This paper contains the results of some of these tests on nine subjects, using 25% NO_2 and 75% O_2, and on three subjects, using other concentrations. Suggestion was not controlled, but the subjects were naïve with respect to the experiment.

The following tests were used:

Sensory

1. *Seashore pitch discrimination.* The subject responded in the usual manner with pencil and paper. The pitch limen was calculated.

2. *Two-point threshold.* The Jastrow-Angell asthesiometer was used. The subject responded by holding up one or two fingers to indicate the number of points felt.

3. *Visual acuity.* The distance in cms. at which a subject could just read a printed line on a visual-acuity card. The subject responded by raising his hand when he could no longer read.

4. *Auditory acuity.* The distance at which a subject could no longer hear the tick of a watch.

Motor

5. *Voluntary tapping.* Whipple's tapping board was used. The number of taps per five-second period were recorded and a curve plotted.

Association

6. *Learning.* A list of nonsense syllables was presented to a subject on a card. He was given one minute to learn as many as possible. A half a minute after the card was taken from him, he was asked to reproduce them on paper.

7. *Free association.* The subjects were given orally 30 words to which they were asked to respond on paper. Time was taken with a stop-watch.

8. *Opposites.* A list of 25 pairs of words, some having the same meaning and some the opposite, were used. The subject responded by writing *S* or *O* beside the paired words.

Affection

9. *Word preference.* A list of 25 paired words, one pleasant and one unpleasant, were handed to the subject with instructions to underline the more pleasant of each pair.

Suggestion

10. *Progressive weights.* The subject is given 10 weights in an order of progressive weights. The first four weigh 10, 25, 50, and 75 grams. All the others weigh 100 grams. The subject is to tell whether the weight he holds is heavier than the one just held.

All of these tests were given under a control and an experimental condition and these conditions were counter balanced in order to correct for practice. The averages and mean deviations of the scores of each test under each condition are given in Table 1.

Subjects tended to fall asleep on the first test or became so drowsy that their responses became mechanical. This is shown in a high limen for the NO_2 group. They came out of this state of drowsiness as soon as another test was presented to them.

Gwathmey (2) suggests that "the senses of the individual are rendered more acute" under the drug. Visual and tactual sensitivity seemed less acute, while auditory was possibly a little more acute under our tests and with our subjects, and provided the differences are assumed to be significant.

In the motor tests, it is interesting to note that, instead of a decrement, there was an increase in the tapping rate in the second half of the period

TABLE 1

Tests	Normal Average	*M.D.*	Nitrous oxide Average	*M.D.*	Unit of measurement
1. Seashore	6.00	3.71	18.00	14.56	vib.
2. Two-point threshold	15.30	3.73	18.70	4.95	mm.
3. Visual acuity	66.70	30.52	64.00	31.20	cms.
4. Auditory acuity	44.50	10.35	47.10	10.6	cms.
5. Tapping (total)	262.80	76.51	199.40	52.8	taps
First half minute	113.75		99.75		
Second half minute	108.35		107.37		
First ten second	45.00		30.00		
Second ten seconds	34.70		34.70		
6. Learning	5.00	1.33	2.10	.83	syllables
(errors)	1.80	.97	2.30	1.80	syllables
7. Free association (time)	8.45	1.25	11.62	1.44	sec.
(total community value)	4033.22	692.48	3407.52	1183.23	freq.
8. Opposites (time)	42.00	6.28	52.35	11.17	sec.
(errors)	.28	.48	1.00	1.44	pairs
9. Word preference (time)	27.06	15.30	45.60	13.68	sec.
10. Suggestion (errors)	2.40	1.75	3.00	1.44	judgment

of tapping under the gas, and that under the NO_2 the subjects failed to show an initial spurt. The total speed of movement, as well as the speed of mental processes, seemed to be considerably diminished, as is shown in all of the measures in this experiment.

In the free-association tests, the community value or the tendency to use a common or trite association was reduced 15.51% under NO_2, in comparing the responses of each subject under both conditions in terms of the Kent-Rosanoff frequency tables (4). Also, the tendency for perseveration (responding with the same word to several stimuli) was increased 3.11 times under NO_2. The associations under NO_2 were more like those of the insane patients.

Test 9, dealing with word preferences, was inserted because of the statement by Moss (5) that "the patient retains control of his intellect, but is entirely without pain or emotional reaction." There was no difference of a qualitative nature found between the normal and experimental groups in this test.

These tests seem to indicate that NO_2 increases suggestibility, and, if this is true, there is a possibility of its use in the psychological clinic.

The common impression that NO_2 is a laughter-producing gas is not borne out. Only one of the nine subjects laughed, and this may have been due to suggestion.

In the experiments to ascertain the efficacy of the use of different concentrations of the gas, the following facts were learned: At 50% concentration and above, there comes a time when the subject can no longer read. Everything looks blurred to him, and certain of the above-mentioned tests cannot

be used for that reason. At this concentration, two subjects failed to show any qualitative differences in association, as compared with the 25% group. The response time grew longer, and the writing became progressively larger and incoordinated, until there was a failure to respond. One subject said that he felt that he was writing very large letters but that, when he looked at them, they were the ordinary size. He said that he felt as though he were "fooling us," in that we thought he was becoming unconscious, but yet he was still able to write.

At 60%, one subject, in responding with free associations to words, began writing progressively larger, and finally was unable to write a single word on the page because of his large movements. Toward the later part of the period, instead of giving an association word, the subject responded mechanically by recording the stimulus word. In responding to opposites by taps—one tap meaning the two words read were the same, two taps meaning that they were opposite—this subject reacted for several minutes intelligibly. Then errors increased threefold, and finally the response became automatic, the subject tapping whenever he heard the experimenter's voice, regardless of the stimulus. Rhythmical movements could be noticed in many of the subjects with all concentrations of NO_2.

Summary

Nitrous oxide anesthesia is an experimental technique which has not been used but which seems to be applicable to many problems in psychology. This paper gives the results of exploratory experiments using representative laboratory tests under different concentrations of the gas. The best concentration for continued partial anesthesia was ascertained, as well as the possibilities for use of other concentrations. Likewise, the most practical avenues of stimulation and response under the gas were learned. Under the gas, free association tended to become more peculiar, all reactions were slowed, learning ability decreased, suggestibility increased, tapping failed to show a decrement, and the senses did not show evidence of being more acute.

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PROTECTIVE COLORATION IN THE LIGHT OF GESTALT THEORY

ANGELINE MYRA KEEN

For many centuries, man, when he gave the subject any consideration at all, was content to say that protective mimicry in the animal kingdom is achieved by virtue of the animal's being merely "colored like its surroundings." That this theory is wholly inadequate and that an animal so colored would be so conspicuous that it would have been eliminated by its enemies early in its evolutionary history, the artist, Abbot Thayer, recognized nearly forty years ago, and, in collaboration with his son, Gerald, published in 1909, in a book entitled *Concealing Coloration in the Animal Kingdom* (5), convincing evidence concerning the true nature of protective coloration. This discovery of his, which in the book is admirably illustrated by many paintings, diagrams, and photographs from life and from models, is in essence:

All objects in natural surroundings are differentially illuminated by the sun, the upper portions receiving maximum and the under portions minimum illumination. Animals (with almost no exception) are counter-shaded, so that the upper parts have maximum and the under parts minimum pigmentation, exactly off-setting the distribution of light. In consequence of this counter-balancing of light and shadow, the animal loses his appearance of solidity and becomes a plane surface which, being quite unlike what the observer (animal or human) is accustomed to in nature, merges with the background. Quoting Thayer:

> "If an object be colored so that its tones constitute a gradation of shading and of coloring *counter* to the gradation of shading and of coloring which light thrown upon it would produce, and having the same rate of gradation, such object will appear perfectly flat:—retaining its length and breadth, but losing all appearance of *thickness;* and when seen against a background of color and pattern like its own will be essentially indistinguishable at a short distance" (5, p. 77).

Again, he says that the animal "has been converted into space and then that space furnished with such material as it should normally contain (5, p. 226).

It is interesting to note the essential equivalence of these two statements, especially of the former, to Koffka's (2) conception of bidimensionality as a special case of tridimensionality.

In the animal kingdom, obliteration by transformation into space is, as Thayer shows, further enhanced by background matching in the form of patterns and markings that are actual pictures of the surroundings in which the animal characteristically lives or in which he is in need of greatest protection. These markings, which are at once highly accurate yet sufficiently generalized to fit the animal's customary environment, Thayer classi-

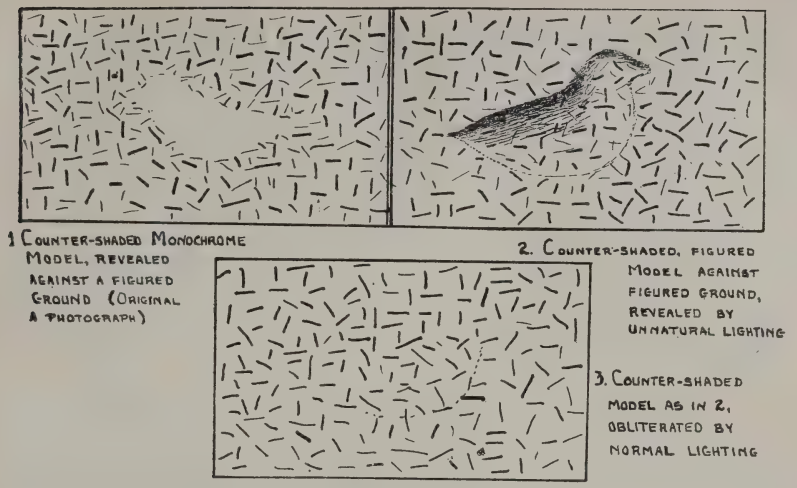
1 Counter-shaded Monochrome Model, Revealed against a figured Ground (Original a photograph)
2. Counter-shaded, figured Model against figured Ground, revealed by unnatural lighting
3. Counter-shaded Model as in 2, obliterated by normal lighting

FIGURE 1
Sketches from Three of Thayer's Illustrations

fied in several broad categories, such as *ruptive,* in which the outline of the animal is broken beyond recognition, *secant,* which cut the animal into bands or stripes, *dazzling,* which distract the eye of the pursuer from the quarry's vulnerable point, and *pictorial,* or the portrayal of the immediate background which the animal invariably affects.

That this conception fits the facts of animal concealment and that such a remarkable type of adaptation could develop through natural selection, biologists now agree and have ceased discussing the subject. It becomes reinvested with interest, however, when considered in relation to modern Gestalt theories. Compare, for example, the following two quotations from Köhler and Thayer, respectively:

> "Even if a stone lies half-embedded in the sand, which is nothing but tiny fractions of the same kind of stone, the difference of coherence, and therefore of 'inner detail,' between the surface-elements of the stone and those of the sand will be sufficient in most cases to make the stone optically one thing. At least, at the boundary between a natural object and its surroundings some discontinuity of properties almost universally prevails. This discontinuity separates the environment from the interior of the object by a closed outline. Since, as a rule, that will suffice to make even a meaningless area appear as a segregated whole in the sensory field, it will certainly have that effect where the boundary of a physical object is concerned. If there are no such differences and no discontinuity whatever between the object and its surroundings, no visual unit will exist, it is true" (3, p. 173).

> "As we have seen, perfect uniformity of coloration makes a thing conspicuous, allowing every part to assume exactly the aspect dictated by its own form, without exaggeration or omission. . . . The main solidity, and its details, are shown by graduated light-and-shade—the outline, the external contours, by relieving either light or dark or differently colored against the background. To all this *markings* are unfriendly, both on objects *actually* monochrome and therefore *visually* not so, and on objects which present, with the aid of counter shading, a perfectly monochrome appearance" (5, p. 77).

It is remarkable that Thayer's account, written from the standpoint of the artist, parallels so closely, in the field of figure and ground perception, the conclusions drawn after prolonged, careful experimentation by such Gestalt psychologists as Rubin, Köhler, Koffka, and Helson. Even the experience-error which Köhler stresses was recognized by Thayer, thus:

> "Other people [non-artists] supplement their undeveloped sight-sense by their other senses, and if they *know* an animal *is* solid, think he *looks* solid" (5, p. 15).

The implication that Thayer should be given the credit for any part of the Gestalt viewpoint is not intended here. As a matter of fact, he was entirely uninterested in the psychological aspects of his discovery, and, though he had a scientific attitude in carrying out his studies, he was by no means a theorist. Rather, the emphasis of this paper is on the fact that, working from a radically different viewpoint, he enunciated a principle which is basically very like the findings of the Gestalter's laboratory—an unexpected confirmation of the Gestalt doctrine.

Interpreting protective coloration in generalized Gestalt terms, we may say that there are two universal *Gestalten* that reign with nature, the one on the side of the predator and the other on the side of the quarry. Each class of animal strives ("strives" in the evolutionary sense of the word) by concealing coloration to incorporate itself into its background—to hide itself completely from the other class of animal by preserving the wholeness of the ground. Equilibrium between these two major *Gestalten* is destroyed when motion on the part of the hunting animal or the prey reveals to the other the existence of a separate figure and is restored for the quarry if, by quick flight or by some ruse, it can elude its enemy, and for the predator if it is successful in capturing its game.

Although pigmentation is the mechanism by which counter-shading is attained, the amount and distribution of pigment is determined not by pure chance but by natural selection, by the stress which has for its equilibrium perfect background-matching. Closure, as far as pigmentation or marking is concerned, is effected when the members of a species become sufficiently obliterated to evade either or both the species upon which they prey or the species which prey upon them. Natural selection, the *modus operandi* by which protective coloration in the animal kingdom is accomplished, is, of

course, the resultant of a number of determining forces. One of these forces, perhaps a major one, we may now recognize as the Gestalt principle of closure or resolution of stress.

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TACTUAL COMPARED WITH VISUAL DISCRIMINATION OF CONSONANTAL QUALITIES*

Milton Weichbrodt

Problem

The problem undertaken here is to compare visual stimulation alone (of a lip-reader) with tactual stimulation alone as means to enable one to discriminate members of pairs of consonantal qualities following long *e*. The problem was suggested by Gault's published data showing a favorable result (as respects the interpretation of spoken language) from enabling subjects at the same time to see the face of a speaker and to feel his words through the teletactor.

This investigation is aimed at finding whether the advantage of dual stimulation may not arise in part from the superiority of touch over vision as a means of discriminating certain consonantal qualities.

Subjects

Three subjects participated in the investigation. They all possess approximately normal sensory capacities as far as known. Two are students and one is an instructor in the Department of Psychology in Northwestern University. All are complete novices in the art of lip-reading. The two

*A Master's thesis in Northwestern University, written under the direction of Robert H. Gault.

students had had no prior experience whatever with the teletactor. The instructor had had no prior experience with the instrument in respect to the specific problem in hand.

Stimuli

The stimuli were 16 consonantal and semi-vowel qualities, each following long *e* as follows: *eeb, eed, eef, eeg, eek, eel, eem, een, eep, eer, eet, eev, eex, eez, ees,* and *eeng.*

Method of Presentation of Stimuli

The stimuli above were arranged in pairs, each one paired with every other, making 120 pairs in all. At the beginning of each laboratory session the subject received a sheet on which five pairs were typed as follows:

1. *eeb eem eel eer een*
2. *eed eek ees eet eep*

As a preliminary the members of the first pair were pronounced, in the order occurring on the sheet, through the teletactor against the finger (the face of the speaker being hidden) or without the teletactor (the speaker's face exposed) for visual stimulation. In either case hearing was completely eliminated by means of a sound-deadening box and an electrically operated sound-barrier. Thereafter 20 pronunciations followed. Throughout, tactual and visual stimulation alternated for first place in relation to each pair of stimuli. The subject then reported for record the order of presentation of members of the pair. The record, therefore, appeared as 2-1 or as 1-2, according to his judgment as to which member was first pronounced. When all was finished, each subject had made 2400 judgments based upon the sense of touch and 2400 based upon vision.

It has been stated that the experimenter *pronounced* the stimuli. This introduced the variations of human speech into the experiment. But there was no practical alternative in the lip-reading situation. And it was therefore necessary to allow the same variations in the tactual as in the visual series. In addition, since the experiments in dual stimulation already referred to employed human speech directly, it was evidently desirable to proceed in the same manner in this case.

Results

The gross results are as follows:

Among the 120 pairs there were 35 the members of which were discriminated more successfully by touch than by vision. In 69 pairs the reverse was true, and in 16 there was no advantage either way. In respect to the 35 cases, there was a grand total of 98 errors by touch (out of a possible 700) and 500 errors by vision (out of 700 possible). In reference to the 69 pairs, 827 errors (out of 1380 possible) were made by touch and 164 (out of a possible 1380) by vision.

The 35 pairs in relation to which the sense of touch proved a more ac-

curate discriminator than vision are as follows: *p-b, b-m, d-t, d-n, d-k, d-f, d-l, d-s, t-n, t-g, t-ng, t-l, t-r, n-g, n-k, n-ng, n-l, n-x, g-k, g-ng, g-l, g-r, k-ng, k-v, k-l, k-r, k-z, ng-l, ng-s, v-f, l-s, r-x, s-x, s-z, z-x.*

The following tables enable us to compare the confusions that were made in the lip-reading situation with those that occurred in the "touch-reading" situation.

Thus Table 1 shows that when the stimulus was *eep-eeb* (or *eeb-eep*)

TABLE 1
PERCENTAGE OF CONFUSION BETWEEN MEMBERS OF PAIRS
Lip-Reading

Stimulus→ ↓	P	B	D	T	M	N	G	K	NG	V	F	L	R	S	Z	X
P		25	0	0	23	0	0	2	0	0	0	0	0	0	0	0
B			2	2	27	0	0	5	2	0	0	0	0	0	0	0
D				30	0	47	42	27	20	2	10	18	8	0	0	0
T					2	23	23	32	27	2	2	43	23	3	0	15
M						0	0	0	0	2	3	0	0	0	13	0
N							40	38	37	0	0	17	2	0	20	7
G								25	57	0	0	15	10	0	2	0
K									47	8	2	12	12	0	2	5
NG										0	3	42	5	15	2	3
V											33	0	2	0	5	0
F												0	0	0	0	0
L													40	22	8	2
R														0	20	5
S															20	40
Z																23

TABLE 2
PERCENTAGE OF CONFUSION BETWEEN MEMBERS OF PAIRS
Tactual Stimulation

Stimulus→ ↓	P	B	D	T	M	N	G	K	NG	V	F	L	R	S	Z	X
P		0	3	33	0	0	0	43	3	0	3	0	0	2	0	7
B			33	0	3	0	20	0	33	9	0	9	3	3	6	10
D				7	7	0	69	0	37	3	3	3	0	0	0	0
T					6	3	17	47	3	0	3	0	0	0	0	36
M						34	10	3	4	27	27	16	36	40	39	0
N							0	3	0	39	16	0	6	10	29	3
G								0	53	0	3	3	3	3	3	0
K									0	0	16	0	0	0	3	10
NG										0	7	6	0	0	17	6
V											0	31	26	10	43	13
F												0	0	32	6	22
L													53	0	33	0
R														11	22	0
S															3	24
Z																0

25% error occurred in the lip-reading situation, etc. Table 2 shows that when the same stimuli were presented in the "touch-reading" situation no errors occurred, etc.

Conclusion

In the experiment alluded to in the opening paragraph the advantage respecting the interpretation of speech of simultaneous visual-tactual stimulation over single visual stimulation was doubtless due *in part at least* to the additional clues afforded by the sense of touch. When visual clues are doubtful, tactual clues may cast the decision. It is inferred that, in the course of the give and take of conversation, a similar result will obtain, assuming appropriate preliminary tactual training.

Note: In a supplementary tactual series the microphone was held, not immediately before the lips of the speaker, but solidly against his larynx. In this situation no instance occurred in which the record equalled that in the lip-reading series. On the whole, moreover, the "larynx" series afforded records that were much inferior to those obtained when the microphone was immediately before the speaker's lips.

AN INTERESTING CASE OF HUMAN BEHAVIOR AFTER LOSS OF BRAIN TISSUE

Paul Chatham Squires

A brief report in the *New York World* for November 12, 1930, set forth the case of Lawrence Beasley of Etowah, Tennessee. It was therein stated that this boy, after having sustained severe cerebral injury through the accidental discharge of his shotgun, still retained his mental powers to a most remarkable extent.

Through the courtesy of Dr. F. O. Nichols of Etowah, the physician in charge of the case, the presentation of the following account has been made possible. Dr. Nichols, in the contents of two letters to the writer of this note, furnished him with all the available facts.

Beasley was an uneducated mountain boy, nineteen years of age. Having gone hunting on the morning of November 10, a grave wound was inflicted in the right side of the head posterior to the ear, in the manner indicated above. The accident occurred at about nine o'clock. Dr. Nichols was called, but, because the injured boy lived in a locality most difficult to reach, did not arrive at the scene of the accident until three and a half hours later. At that time Beasley had not yet been removed from the place of the accident.

Dr. Nichols, in his first letter, dated November 17, states:

> "My first observation was a tumor of brain tissue, the approximate size of a hen's egg, surrounded by blood clots and external to the wound. Upon the removal of this, a further

> examination revealed an opening through the skull into the brain slightly less than the size of a silver dollar. I stopped the hemorrhage, used compresses and supportive treatment. My second visit was made at 6 P.M. At this time there appeared a second tumor or exudation of brain tissue of from one half to one ounce in weight. I did not disturb this, since the hemorrhage had ceased. I still felt that the boy had no chance to live. Another visit was made on the morning of November 13th, at which time I decided that his general improvement warranted his being sent to a hospital. This was accordingly done and an operation performed under general anesthesia. From three to four tablespoonfuls of brain tissue were removed. The wound was located at the juncture of the parietal, temporal, and occipital bones: that is to say, at the asterion. Before sending the boy to the hospital I had removed a piece of the cranium one and a half inches square. During the operation at the hospital three more pieces were removed."

At the doctor's first visit Beasley was, of course, in a state of considerable shock. He had lost much blood. The pulse was weak, thready; rate, 140. "But the patient was entirely conscious and rational." The people who found him maintained that at no time had he been unconscious, at least as far as their observations went. When the doctor arrived, the first question Beasley asked was: "Who is this?" Furthermore, he wished to know whether he was badly hurt. After a conference with the boy's mother and brother, it was decided to tell him that he would probably not recover. Upon hearing this, Beasley began to pray and shortly thereafter made the statement that he thought he was saved. He said to his brother that the Lord had told him he would get well.

The doctor's second visit, made about nine hours after the accident, showed no alteration in the general condition of the patient. Beasley was fully conscious and intensely interested in the outcome. Dr. Nichols writes:

> "The only abnormalities noticed were a slight deafness which, according to relatives, had not been present before the accident, and a cerebral depression. The pupils were slightly contracted, responding sluggishly to light. Reflexes other than pupillary were normal. There were no speech defects. However, when asked a question, Beasley would say 'huh' and then would answer only after two or three seconds had elapsed."

Death occurred on December 31, fifty-two days after the accident, the proximate cause being cerebritis. There had been very little change in Beasley's mental condition until three or four days before the end, at which time he sank into a semicomatose state; he was then also subject to periods of delirium. During these last few days the following reflex peculiarity was observed: the pupils would contract fairly well in response to light, but, almost immediately after contraction, would dilate, although the light had not yet been removed.

As time went on there was observed a slight increase in deafness. How-

ever, Dr. Nichols inclines strongly to the opinion that this deafness was not so much sensorial as a general cerebral retardation in grasping questions.

Dr. Nichols has estimated that between four and six ounces of brain tissue were lost. In this loss the gray matter predominated. No post-mortem examination of the brain was carried out. Hence, the inquiry made by the writer as to the exact cerebral location of the injury could not be satisfied. However, it is known that the second temporal gyrus lies over the region of the asterion.

Dr. J. W. Papez, Professor of Anatomy at Cornell University, who is particularly interested in the problem of cortical function and localization, has been so good as to examine the preceding account of the case submitted to him through the courtesy of Professor H. P. Weld. Dr. Papez has drawn therefrom the inference that the loss of brain tissue was from the "posterior halves of the right second temporal convolution just behind the definitive auditory area."

Dr. Papez goes on to say:

> "The (Beasley) case supports the view that the auditory functions have a bilateral representation.....It has been shown from similar cases that large cortical loss in the right hemisphere may, in right-handed individuals, interfere very little with the integrative functions of the cortex.....With some exceptions, lesions of the left hemisphere usually produce more definite symptoms.....The only lesions of a nature resembling this case which have definitely abolished consciousness are those located in the median side of the left frontal lobe" (1).

Piéron has stated:

> "A destruction of both temporal auditory areas is necessary in order to bring about immediate deafness:....cortical deafness of one area only is unknown" (2, pp. 26-27).

Although it is regrettable that there is no direct evidence at hand in this case concerning the exact locus of the brain injury, nevertheless, the fact should be stressed that mere topographical relation is not, after all, the controlling factor in the matter of cerebral localization. It is doubtful, to say the very least, whether cerebral centers are determined in anything like a detailed manner by the specific distribution of sulci and convolutions. Piéron has well pointed out in this connection:

> "In fact cerebral architectonics, the histological study of the structure of the layers of the cortex, has established the fact that the functional areas correspond to the presence of certain arrangements of cells; *and these functional areas have proved capable of considerable individual variation*" (2, p. 32).[1]

The Beasley case, then, although in no way offering novel data on the problem of the localization of cerebral functions, does seem to furnish one more instance in favor of the doctrine of bilateral auditory representation.

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[1](Italics are the writer's.)

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THE CHANCE ELEMENT IN THE MULTIPLE CHOICE TEST ITEM

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Suppose a multiple choice test item has alternatives $A_1, A_2, \ldots\ldots A_n$. The proportion of an infinite number of individuals who mark each alternative will be indicated by $S_1, S_2, \ldots\ldots S_n$, where the S's are arranged in ascending order of magnitude. We assume that in the majority of cases we may usefully make the following definition:

> The greater the proportion of individuals who mark a given alternative the closer does that alternative come to being the true answer.

Let us define the difficulty of each alternative as the amount of ability required to distinguish it from the true answer, and let us assume that the range of difficulty of the alternatives is included within the range of ability of the group.

Having given the proportion of individuals who mark each alternative, we wish to determine how much of each proportion is composed of individuals who mark the alternative solely by chance.

We may indicate by $X_1, X_2, \ldots\ldots X_n$, the degree of ability required to distinguish between the respective alternatives and the true answer. We indicate by P_0 the proportion of individuals whose ability is less than X_1, by P_1 the proportion of individuals whose ability is between X_1 and X_2, by P_2 the proportion of individuals whose ability is between X_2 and X_3, and finally, by P_n the proportion of individuals whose ability is greater than X_n.

Now the individuals in the P_0 group will mark entirely according to chance, that is, $\frac{P_0}{n}$ will mark each alternative. The individuals in the P_1 group will not mark the first alternative but they will mark all of the others according to chance, that is, $\frac{P_1}{n-1}$ will mark each alternative from 2 to n. Hence, in general, we may write

$$S_1 = \frac{P_0}{n}$$

$$S_2 = \frac{P_0}{n} + \frac{P_1}{n-1}$$

$$S_3 = \frac{P_0}{n} + \frac{P_1}{n-1} + \frac{P_2}{n-2}$$

. . . . . [1]

$$S_{n-1} = \frac{P_0}{n} + \frac{P_1}{n-1} + \frac{P_2}{n-2} + \ldots\ldots + \frac{P_{n-2}}{2}$$

$$S = \frac{P_0}{n} + \frac{P_1}{n-1} + \frac{P_2}{n-2} + \ldots\ldots + \frac{P_{n-2}}{2} + \left[P_{n-1} = P_n \right]$$

The expression in brackets in the last equation means that A_n will be marked by all individuals between X_{n-1} and X_n in ability as well as those above X_n. In other words, all individuals above X_{n-1} in ability will mark A_n.

In the group of individuals who know the best answer we shall include both P_{n-1} and P_n. All other individuals mark A_n by chance. From the last two equations in [1] we get

$$S_n - S_{n-1} = P_{n-1} + P_n \qquad [2]$$

but $P_{n-1} + P_n$ is the proportion of individuals who know that A_n is the best answer. Hence, the proportion who mark A_n by chance is simply S_{n-1}.

Suppose now that

$$X_1 = X_2 = \ldots\ldots = X_{n-1} \qquad [3]$$

which, according to our analysis, can be true only when

$$S_1 = S_2 = \ldots\ldots = S_{n-1} \qquad [4]$$

We define

$$D = P_{n-1} + P_n \qquad [5]$$

$$R = S_n \qquad [6]$$

$$W = S_1 + S_2 + \ldots\ldots S_{n-1}$$

or, from [4]

$$S_{n-1} = \frac{W}{n-1} \qquad [7]$$

Substituting [5], [6], and [7] in [2], we get

$$D = R - \frac{W}{n-1} \qquad [8]$$

It will be seen at once that [8] is the formula used to correct for chance effect when scoring multiple choice tests, where

D = score
R = number of items correct
W = number of items incorrect
n = number of alternatives

According to the logic by which [8] was derived it is clear that the formula is valid only when all the alternatives of a given test item, not

including the best answer, are of equal difficulty. In most tests this condition does not hold. However, if we know how much greater S_{n-1} is than $\frac{1-Sn}{n-1}$ we can determine the number, r, of alternatives which would give the same chance effect, as would obtain if

$$S_1 = S_2 = \ldots\ldots = S_{r-1} < S_r$$

The condition to be satisfied is that

$$\frac{1-S_n}{r-1} = S_{n-1} \qquad [9]$$

We let

$$S_{n-1} = \frac{1-S_n}{n-1} + a \qquad [10]$$

where a is the deviation of S_{n-1} from $\frac{S_1 + S_2 + \ldots\ldots + S_{n-1}}{n-1}$

From [9] and [10] we obtain

$$\frac{1-S_n}{r-1} = \frac{1-S_n}{n-1} + a$$

or

$$r = 1 + \frac{(1-S_n)\ (n-1)}{(1-S_n) + a(n-1)} \qquad [11]$$

From [11] we obtain

$$n-r = \frac{a(n-1)^2}{(1-S_n) + a(n-1)} \qquad [12]$$

Inspection of [12] shows that r can never be greater than n. It is always less than n unless a is zero, that is, unless

$$S_1 = S_2 = \ldots\ldots = S_{n-1}$$

If r, as obtained from equation [12], were approximately uniform for all the items in the test, this value might be substituted for n in Equation [8] in order to correct the score for chance effect. However, if r varies considerably from item to item, no general correction formula can be used.

For this reason it is always important to approximate as closely as possible one of the following conditions:

1. All the alternatives except the best answer should be of equal difficulty.
2. The alternatives should be uniformly spaced on the difficulty continuum.

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A FORMULA FOR A MEAN TETRAD

CECIL R. BROLYER

In problems involving tetrad analysis, frequent use is made of the mean tetrads arising from a combination of tests in each of which the correlation between a certain pair of tests is always on the same side of the negative sign. It is often advantageous to calculate the mean tetrads before the individual ones have been obtained, particularly in those situations in which the interpretation of the mean tetrads cause the subsequent procedure to be altered in such a way that certain individual tetrads are not desired. Even if all the latter have been calculated first, the signs need to be handled with care in taking out the mean tetrads.

The following formula is an expression for such a mean in terms of the original correlations, and its calculation is a straightforward operation on a computing machine. In the development that follows, no r has the two subscripts the same, that is, in the complete rectangular table of correlation coefficients one main diagonal is composed of blank cells.

If there are n tests, the mean of the $(n-2)$ $(n-3)$ tetrads, each of the form $(r_{ab}r_{ij} - r_{ai}r_{bj})$, given by the two fixed tests and all other pairs of tests is:

$$M_{t\ abij} = \frac{\sum_{\substack{i,j=1 \\ i,j \neq a,b}}^{i,j=n} (r_{ab}r_{ij} - r_{ai}r_{bj})}{(n-2)\ (n-3)} \qquad [1]$$

If the summation be taken separately with respect to each of the two terms of the numerator, that to the left of the negative sign may be written immediately as:

$$r_{ab} \sum_{\substack{i,j=1 \\ i,j \neq a,b}}^{i,j=n} r_{ij} = r_{ab}\left[\sum_{i,j=1}^{i,j=n} r_{ij} - 2\sum_{i=1}^{i=n} r_{ai} - 2\sum_{i=1}^{i=n} r_{bi} + 2r_{ab}\right] \qquad [2]$$

The term to the right of the negative sign in [1] is perhaps most easily written down after observation of the forms in which the r's occur. Consider the series of tests, $a, b, c, d, \ldots\ldots m, n$, in which we are interested in the mean tetrad containing the pair ab. There are $(n-3)$ pairs of terms of the form $r_{ac}r_{bi}$, $r_{ai}r_{bc}$. There are $(n-4)$ pairs of terms of the form $r_{ad}r_{bi}$, $r_{ai}r_{bd}$. Finally, there is one pair of the form $r_{am}r_{bn}$, $r_{an}r_{bm}$. This gives us $(n-2)$ $(n-3)$ terms which may be regrouped and thrown into $(n-2)$ sets, each of the form:

$$r_{aj} \sum_{i=1}^{i=n} (r_{bi} - r_{ab} - r_{bj}),\ (j = 1, \ldots., n, j \neq b)$$

If the $(n-2)$ sets be summated after the indicated multiplication has been

carried out, the quantity on the right of the negative sign in [1] becomes:

$$\sum_{i=1}^{i=n} r_{bi} \sum_{i=1}^{i=n} r_{ai} - r_{ab} \sum_{i=1}^{i=n} r_{bi} - r_{ab} \sum_{i=1}^{i=n} r_{ai} + r^2_{ab} - \sum_{i=1}^{i=n} r_{ai} r_{bi} \qquad [3]$$

If [3] be subtracted from [2] and the result divided by $(n-2)\ (n-3)$, the expression for the mean tetrad is obtained as:

$$M_{t_{abij}} = \frac{\sum_{i=1}^{i=n} r_{ai} r_{bi} + r_{ab} \sum_{i,j=1}^{i,j=n} r_{ij} + r^2_{ab} - \sum_{i=1}^{i=n} r_{bi} \sum_{i=1}^{i=n} r_{ai} - r_{ab} \sum_{i=1}^{i=n} r_{ai} - r_{ab} \sum_{i=1}^{i=n} r_{bi}}{(n-2)\ (n-3)} \qquad [4]$$

Before beginning the calculations, it is probably best to fill the rectangular table of correlations completely except for the one main diagonal. Next write down the sum of the correlations in each column. These sums correspond to the terms $\sum_{i=1}^{i=n} r_{ai}$. The term $\sum_{i,j=1}^{i,j=n} r_{ij}$ is the same for all mean tetrads and is the sum of the sums of the separate columns. Next obtain the value of $(n-2)\ (n-3)$.

Multiply successively each correlation in column a by the corresponding correlation in column b and allow the products to accumulate in the machine. Multiply into the machine $\sum_{i,j=1}^{i,j=n} r_{ij}$ by r_{ab}, and r_{ab} by itself; multiply out of the machine successively $\sum_{i=1}^{i=n} r_{ai}$ by $\sum_{i=1}^{i=n} r_{bi}$, $\sum_{i=1}^{i=n} r_{ai}$ by r_{ab}, and $\sum_{i=1}^{i=n} r_{bi}$ by r_{ab}. Divide the result in the machine by the previously determined $(n-2)\ (n-3)$; the result is the mean tetrad.

Example

	Test 1	Test 2	Test 3	Test 4	Test 5	
Test 1		.9156	.7270	.7298	.8810	3.2534
Test 2	.9156		.7276	.7213	.8677	3.2322
Test 3	.7270	.7276		.8181	.6935	2.9662
Test 4	.7298	.7213	.8181		.6628	2.9320
Test 5	.8810	.8677	.6935	.6628		3.1050
	3.2534	3.2322	2.9662	2.9320	3.1050	15.4888

In this example, $(n-2)\ (n-3) = 6$, and $\sum_{i,j=1}^{i,j=n} r_{ij} = 15.4888$. If the mean of the tetrads arising from the pair of Tests 1 and 2 is desired, $r_{ab} = .9156$, $\sum_{i=1}^{i=n} r_{ai} = 3.2534$, and $\sum_{i=1}^{i=n} r_{bi} = 3.2322$. Multiply into the machine

successively .7270 by .7276, .7298 by .7213, .8810 by .8677 (1.8198 is $\Sigma_{i=1}^{i=n} r_{ai} r_{bi}$), 15.4888 by .9156, and .9156 by itself. Multiply out of the machine 3.2534 by .9156, 3.2322 by .9156, and 3.2534 by 3.2322. Divide the quantity in the machine (.3858) by 6, and $.064 = M_t$.
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MOTIVATION AND MAZE-LEARNING: A REPLY TO PROFESSOR TOLMAN

C. J. WARDEN

In a recent volume of this journal (*J. Gen. Psychol.,* 1930, **4**, 338-342), Professor Tolman offers a criticism of what he conceives to be the current theory of maze-learning and suggests a revision of this theory in view of recent findings which emphasize the importance of motivation in the maze-learning situation. The paper had been read before the Ninth International Congress of Psychology, at New Haven, in 1929. The present article represents a somewhat modified and abridged reply to Professor Tolman's paper, prepared prior to and read at the same session of the congress above mentioned.

Professor Tolman begins by stating that the classical account of maze-learning is too simple since it "conceives of the rat as responding to a mere series of moment-to-moment stimuli," and does not place sufficient emphasis upon motivation factors. He then reviews the results of a number of motivation studies, in which the maze was utilized, and summarizes the bearings of these on the nature of maze-learning by relating them to his own theory. The latter recognizes maze-learning in the rat as involving the following four independent variables: (1) knowledge of the maze path, (2) motivation as represented by such physiological states as hunger, thirst, etc., (3) the goal object, or incentive, and (4) the reward-value of the goal-object, or incentive, as dependent upon its appropriateness to the other aspects of the situation. He regards such an analysis as anthropomorphic but believes it to be more adequate, considering our present state of ignorance, than other analyses based upon "Thorndikian bonds" or "Gestaltian configurations." In this reply, we shall consider, first of all, the criticism of the "classical" view which Tolman makes, and then pass to the question of his own analysis of the maze-learning process.

The essential implication of the criticism seems to be that the students of animal behavior of a generation ago, from whose work we derive our present conception of the nature of maze-learning, either denied the importance of motivation outright or ignored it almost entirely. This seems

to be a very unfair statement of the case, however. In looking through the early work of Small, Kline, Thorndike, Watson, Carr, and other workers, one nowhere finds the notion presented that maze-learning is merely a response to moment-to-moment stimuli. It is true that the motivation factor was sometimes taken for granted and only mentioned casually, but that is far from a denial of its importance. All of these workers explicitly recognized that motivation factors were basic and essential in that they all worked with animals that were hungry, thirsty, etc. In fact, Carr was the first to show that an overfed animal makes little or no progress in the formation of a maze habit. Thorndike had much to say of impulse and of its importance in learning in connection with his very first experiments. Even the frequency-recency theory of Watson—which is by all odds the most mechanistic theory of all—presupposed the use of a properly motivated organism. This point is stressed by Watson, in replying to the critics of his theory, in the chapter on learning in his *Psychology from the Standpoint of a Behaviorist.* In fact, the basic importance of instinct, *Triebe,* inner physiological states, etc., to learning was so well recognized by these early workers that no one in those days thought of making an issue of it.

It is true that the early maze studies had much to say concerning the stimulus aspect of the process involved. But this is to be accounted for by the fact that the question of the nature of the sensory cues in maze performance was uppermost at that time. Perhaps the emphasis has now shifted to other factors such as motivation, but this does not mean that sensory factors were regarded as the *sole factors* during the earlier period. Both lines of analysis are of value in giving us insight into the nature of the maze problem. If we examine carefully the facts cited by Tolman, growing out of recent studies of motivation, we shall find that these are not of a crucial character in themselves. Any of them can be easily enough fitted into the older as well as the newer concepts of learning. They are not at all upsetting to the current association theories of learning. Reward-values, as well as the other categories of Tolman's analysis, can be stated readily enough in terms of "Thorndikian bonds" if one wishes to take the trouble to do so. The findings discussed by Tolman are merely a part of that increasingly large body of experimental literature which seeks to analyze the motivation factor in learning somewhat more specifically than the earlier workers seemed inclined to do.

Perhaps everyone will agree that a more explicit analysis of the motivation factor in this and other types of learning is extremely desirable. The first three terms of Professor Tolman's analysis, however, have been widely recognized as such and definitely stressed in recent years at least; no one would think of writing up a learning experiment without dealing with the sensory cues, drive status, and incentive conditions involved. These common terms correspond respectively to the knowledge, motivation, and goal-object factors of Tolman's analysis. The only term that requires special

attention, therefore, is Tolman's fourth, which takes account of the reward-value of the goal-object. The essential aspect of this concept is the matter of the appropriateness of the incentive to the several factors covered by the other three terms. But even this concept is not new. Surely, it is generally recognized that food must be used as the incentive if you are testing the learning capacity of a hungry rat. Naturally enough, if you shift suddenly to the thirst drive you must put water in the goal box instead of food. I fail to see anything upsetting about results of this sort. In fact, I feel certain that Thorndike, Watson, Carr, and the rest would say that a certain appropriateness in the incentive-drive situation must be taken for granted as essential. Doubtless, there is some advantage in making the factor of appropriateness one of the terms of the analysis, since it thus becomes more explicit. But there is nothing new in the concept itself.

Professor Tolman accuses himself of anthropomorphism—especially in connection with the use of the notion of reward-value, or appropriateness of the incentive. In the strict sense, however, the notion of appropriateness is teleological rather than anthropomorphic. I do not see that it is any more teleological than such concepts as impulse, *Triebe,* instinct, drive-incentive, etc., which must imply a certain appropriateness in order to possess any biological meaning. If we regard appropriateness of incentive as merely one aspect of biological adaptation—and surely all will agree to this—then it ceases to be teleological in the bad sense. At any rate, unless the appropriateness here meant is couched in mentalistic terms, it is obviously not anthropomorphic. In general, the notion of reward-value seems to me to be much more objective and useful than such concepts as "satisfyingness" and "unsatisfyingness" as used by Thorndike to take account of approximately the same factors in learning.

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PERCEPTUAL INSIGHT AS IT IS RELATED TO ACADEMIC CLASSES: AN EXPERIMENTAL STUDY

GEORGE F. DUNKELBERGER AND ELDON K. RUMBERGER

If insight be an important factor in the psychic life of the individual, it seems only logical to presume that its development should proceed at a rate commensurate with the growth of the other bodily functions. No assertion is made that this is true; however, we raise this question as a point about which we may build our discussion and as an angle from which we may present our experimental discoveries. In short, we wish to discover just what insight may be and how it relates itself to our known laws of learning. As we frankly stated in a previous article, this whole matter of Gestalt is somewhat of an enigma. If, by any experimental data, it be

possible to show the relationship between insight and learning without an absolute interdependence, then Gestalt becomes less of a puzzle.

With this aim in view, the following experiment was conducted. The subjects in this study numbered 1469 from all classes of eight Pennsylvania colleges. The tests were given to groups of students ranging in size from 40 to 150. They consisted of fitting together (by means of mental imagery) numbered segments of lettered geometric figures to form the figures themselves. (See Figure 1). The answers to the tests were written on test slips especially prepared and were expressed in numerals. The subjects were not permitted to draw the figures on paper or in any other way aid themselves in the solution of the problem by trial and error. They were instructed to write down the numbers of the nine dissected forms which when placed together would make up the three respective lettered geometric forms, e.g., A—1, 4, 9, etc. The subjects were informed that no segment

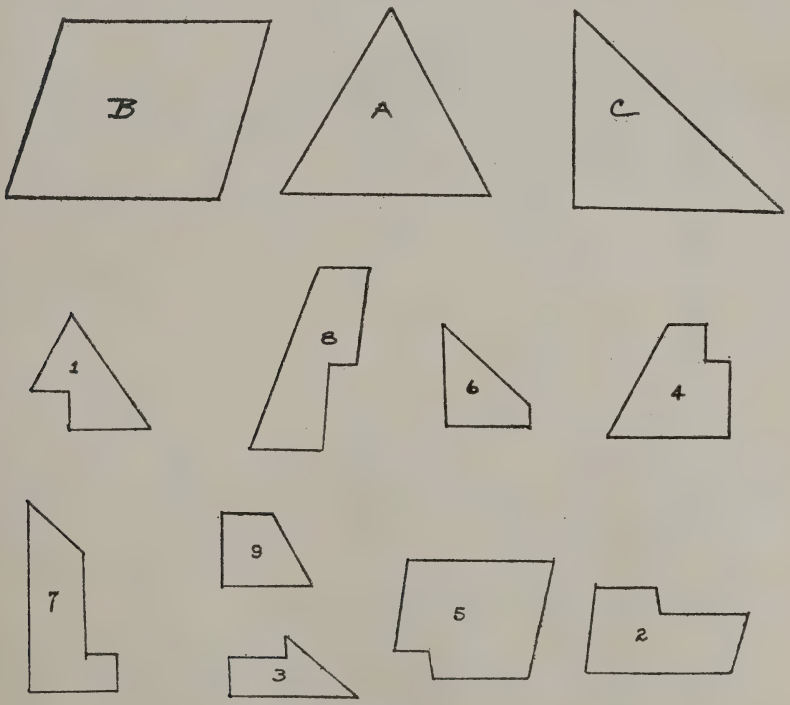

FIGURE 1

REPRESENTING THE TEST CARD WHICH WAS DISPLAYED FOR SOLUTION IN THE EXPERIMENT

(The answers were written in numerals, e.g., A=1-4-9; B=8-5-2; C=3-6-7.)

TABLE 1

Age	Score			
	0	1	2	3
16	33	8	0	59
17	18	23	3	56
18	28	28	5	39
19	19	29	2	50
20	20	27	4	49
21	21	20	1	58
22	21	23	1	55
23	19	41	7	33
24	27	16	6	51
25	21	30	12	37
26	30	30	6	34
27	27	20	3	50

TABLE 2

Age	Score			
	0	1	2	3
16	1	2	0	1
17	7	6	7	9
18	17	14	20	10
19	18	24	15	21
20	17	20	20	18
21	15	11	7	17
22	8	8	2	10
23	4	7	9	3
24	3	2	5	2
25	2	2	9	2
26	1	1	2	1
27	7	3	4	6

could be used more than once to complete the lettered figure. The results of these tests were tabulated on a form as given below (Chart 1). In this system of tabulation, 3 represents a perfect score, as there were but three geometric forms to be solved.

The results of these tests may be observed in Tables 1-4 from which we take the liberty to draw the following observations:

Table 1 reads that 33% of the students sixteen years old scored 0; 8% scored 1; none scored 2; and 59% made a perfect score. Table 2 means that 1% of all the students making a zero score were sixteen years old; 2% of all the students making a score of 1 were sixteen years old; none of all the students making a score of 2 were sixteen years old; and 1% of all the students making a perfect score were sixteen years old. In like manner Tables 3 and 4 should be read.

CHART 1

Age and sex		Score 0				1				2				3			
		Fr.	So.	Jr.	Sr.	Fr.	So.	Jr.	Sr.	Fr.	So.	Jr.	Sr.	Fr.	So.	Jr.	Sr.
16	M	3			2		2			1			2		3		8
	F		4			4					4			1		6	
17	M				18				3				3				
	F											1			7		1

TABLE 3

Age	Sex	Score 0	1	2	3
18	M	25	19	5	51
	F	30	30	5	35
19	M	12	26	2	60
	F	23	32	2	43
20	M	20	24	4	52
	F	21	31	3	45
21	M	26	21	0	53
	F	14	18	4	64
22	M	20	23	0	57
	F	24	24	3	49
23	M	16	40	10	34
	F	25	45	0	30

TABLE 4

Students	Score 0	1	2	3
442 Freshmen	27	30	4	39
357 Sophomores	15	27	3	55
393 Juniors	17	20	2	61
277 Seniors	26	25	4	45
1469 Total	21	26	3	50

1. The factor of insight is most prominent among students falling within standard school ages as shown by the results in Table 1 and Table 2.

2. Boys display a greater degree of insight than girls as shown in Table 3.

3. The decline in the degree of insight displayed by college students seems to become apparent in the senior year. The highest scores were made by juniors. See Table 4.

4. From these results we are unable to explain the case of those subjects scoring two. Inasmuch as they were informed that no segment could be used more than once, we are at a loss to understand the failure to solve the third lettered figure by a process of elimination. Most of those subjects scoring 2 did so because they left the third figure unsolved. On the face of the matter we feel to suggest that the amount of insight necessary to solve the problem by elimination was lacking. From here we may go no further. We can but report this experiment and its attendant results.

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APPARATUS

A ROTARY ELECTRIC SWITCH

Stanley B. Lindley

A quick-acting electric switch was designed to respond with the lowest possible latency to changes in the direction of motion. Its use is indicated when the technical problem is to record electrically any changes in direction of more or less extensive oscillatory movements, irrespective of their amplitude. In other words, this switch emphasizes the beginning of processes and ignores their amplitudes. It is what might be termed a mechanical systematic leak. In addition, it is relatively simple and remains in adjustment throughout extensive periods of use. The rotary switch has been used for the recording of bodily restlessness, finger-oscillation, and the r.p.m. of an electric motor.

The switch consists essentially of a small brass cylinder *S*, Figure 1, which carries a metal contact arm *A* at right angles to its axis. This cylinder rotates on cone bearing and gives the distal end of the contact arm an amplitude proportional to its length. The arm moves between the two adjustable contact points *P* and P_1, and when it strikes either of these points it closes the electrical circuit of which that contact is a part. The current through the switch passes through bearing-yoke *Y*, brass bearing points, cylinder *S*, arm *A*, and contact point *P* or P_1.

Excess motion is taken up by the manner of transmitting it to the cylinder. It preferably consists, as shown in Figure 1, of a cord *C* running from the organ or mechanism whose movement is being investigated over the cylinder. When the axis lies in a horizontal plane, the cord, kept at a slight tension by a small weight, produces just enough friction to turn the switch, breaking or making the contact at the least change in the direction of motion.

The switch is an outgrowth from the optical lever devised by Professor Raymond Dodge which is used in conjunction with his photographic technique for the magnification and registration of movements of extremely small amplitude.

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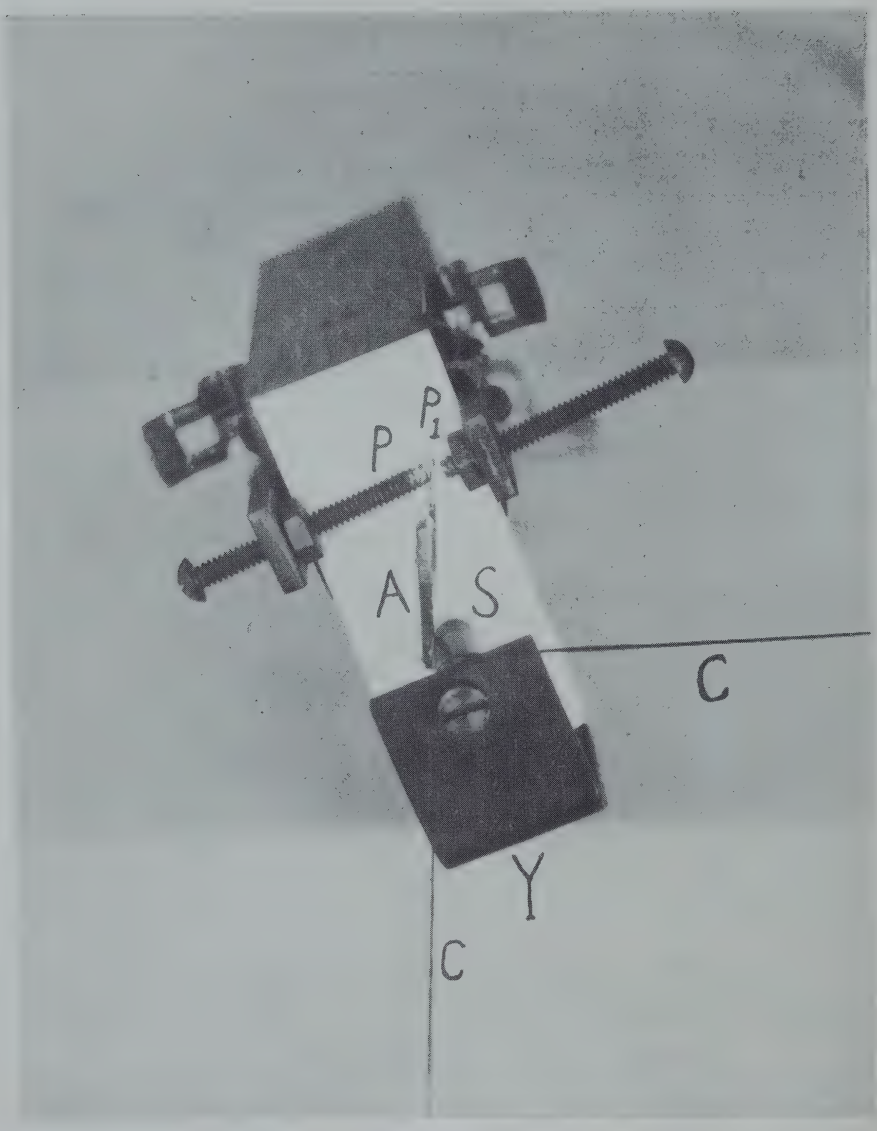

FIGURE 1

A NEW FORM OF TACHISTOSCOPE

HAROLD GULLIKSEN

In connection with my thesis it became advisable to develop an apparatus which would satisfy the following requirements:

1. Present a series of visual stimuli to the subject, the series being of any desired length.
2. The time between stimulations should be constant for any series, and yet be easily altered for different series.
3. The duration of each stimulation should be constant for any series, and yet be easily altered for different series.
4. The time between stimulations, and the duration of each stimulation, should be independently variable.
5. It should be easy to vary the order in which the stimuli are presented.
6. After exposure to the subject, the series should be in the same order again, ready for presentation to another subject.

The plans of the apparatus were developed at Ohio State University under the direction of Dr. Samuel Renshaw. The construction work was started at Ohio State in 1929 and completed at the University of Chicago in 1930. I also wish to thank Mr. Hampton, the mechanic for the psychology department at Ohio State, and Mr. Larson, the mechanic for the psychology department at the University of Chicago, for construction work on the apparatus.

An apparatus satisfying the above requirements would be valuable for a wide variety of problems, such as work on memory, retention, recognition or recall or any type of visual pattern within the size limits of the cards used in the machine; work on concept formation of the type done by Kuo, Fisher, Hull, and Moore; and work on the development of language. In fact, such a machine would be of use in any type of experimentation requiring the presentation of a series of visual stimuli where control of order and time of presentation is desirable. It is suitable for either very short or long exposure times.

In the present experiment, it was also desirable to have the cards containing the stimuli determine automatically the warnings given the subject whenever he made an incorrect response. This feature would be of value whenever the experiment called for a discriminative reaction on the part of the subject, and the experimenter wished to warn or punish the subject when errors were made.

The figures or words to be used are printed or drawn on cards (5″ x 6″ x 1/16″) made of a very hard, durable gray fiber board that takes printing very legibly. If unusual forms are to be used, they may be drawn on the cards with India ink or cut in linoleum blocks and then printed on the cards.

The machine used to expose these to the subject consists of the following parts:

(*A*) a hopper to hold the cards before exposure. This is made of four 18″ lengths of angle brass and is 5 1/16″ by 6 1/16″ by 18″. This hopper is bolted to

(*B*) a metal plate containing a groove 1/16″ deep, 6 1/16″ wide, and 10″ long. The card at the bottom of the pile in hopper (*A*) lies in this groove.

(*C*) a pusher foot of brass .06″ thick 6 3/64″ long and 6″ wide slides

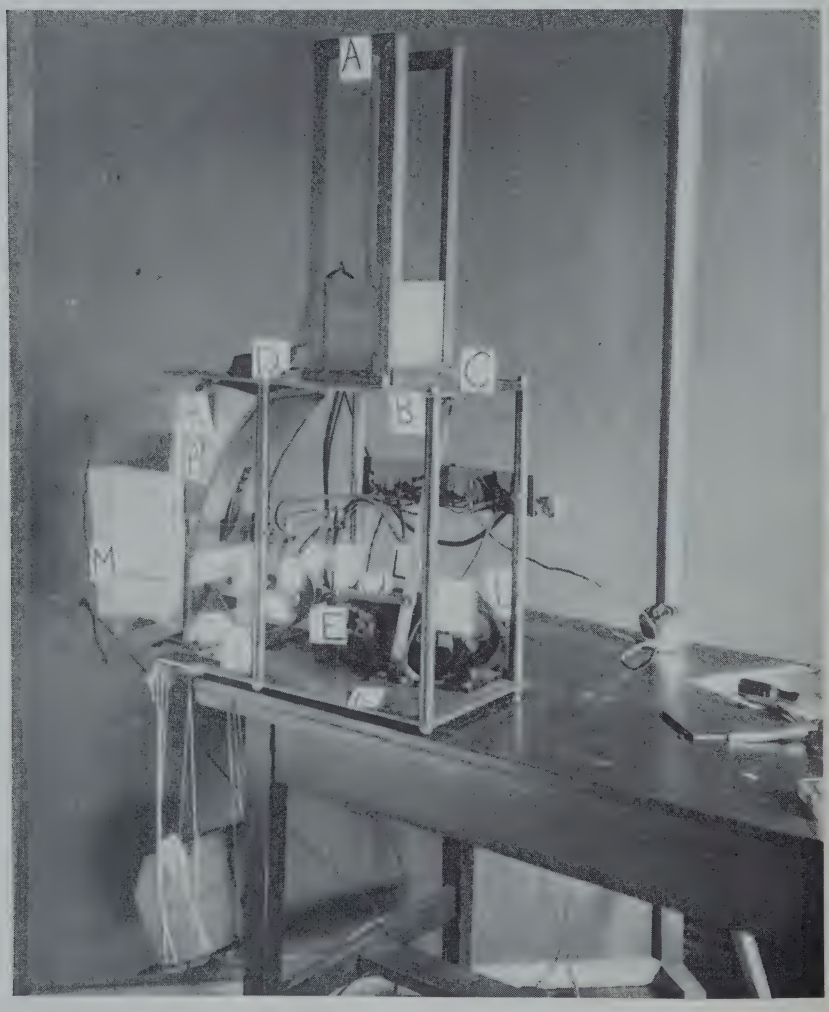

FIGURE 1

back and forth in this groove and pushes out one card at a time from the bottom of the stack, through

(*D*) a throat at one end of the groove. This throat is 3/32″ high, and 6 1/16″ long, that is, it is so narrow that only one card can go through at a time.

The pusher foot (*C*) is moved by

(*E*) an eccentric fastened to

(*F*) a camshaft mounted on

(*G*) two brass bearings.

The camshaft (*F*) is turned by

(*H*) a 1/20 h.p., 1725 r.p.m. motor working through

(*J*) a 100 to 1 gear reducer.

The camshaft makes one revolution in 2.9 seconds.

The motor (*H*), camshaft bearings (*G*), and gear reducer (*J*) are all bolted to

(*K*) the plate which forms the base of the apparatus.

It is possible to obtain a wide range of speeds by slipping a different sized pulley (*L*) on the shaft of the motor or of the gear reducer, or both.

The camshaft (*F*) also operates three contacts,

(N1), (N2), (N3), by means of three cams (O1), (O2), (O3). Each of these three contacts may be made for any desired fraction of a cycle by using a cam of the proper size. In the present set-up, two of these contacts are used for sounding a buzzer whenever the subject makes an incorrect response; and one of the contacts is used to operate a light which illuminates the card.

When pushed through the slot (*D*), the card falls down into (*P*) a slot made of two 16″ pieces of 1/4″ by 3/8″ channel brass. These pieces were split from the top down to 5″ from the bottom, and the split part spread out as shown in order to allow the card to turn gradually from the horizontal position which it occupies in the hopper (*A*) to the vertical position in which it is exposed to the subject. Sheets of tin were soldered to the split channel brass to keep it in shape. The tin was then covered with cardboard and cloth to deaden the noise of the falling cards. This entire arrangement is pivoted to the upper plate (*B*) at (*R*).

The card, after being pushed out from the bottom of the stack slides down through this slot (*P*) and comes to rest on the bottom plate (*K*). Here it is encased in

(*M*) a box which admits very little light, (The room is darkened during the experiment) and is open only in front towards the subject. The card is then "exposed" to the subject by a light concealed in (*M*). This light is operated by one of the cams and contacts as explained above.

(*T*) represents one of two light spring contacts placed on plate (*K*), so that the weight of a card will close both contacts, unless one or two notches are cut in the card to prevent it from making either one or both of the contacts. These contacts operated by the cards determine (in conjunction

with the reaction of the subject) the warning signals. Other contacts might be used if desired, and, with the cards notched out in different ways, it would be possible to have the cards themselves control any special arrangements of warnings, punishments, or other signals that were to be given with a particular response to a given visual stimulus.

When the period of exposure of a given stimulus is ended, the light goes out, and then

(*U*) a cam pushes the holder (*P*) forward, so that the card is no longer resting on plate (*K*) which forms the base of the apparatus. The card now drops into

(*V*) a holder where the cards are restacked in the same order as they were in the hopper (*A*) above.

This apparatus makes it possible to present any type of visual pattern, within the size limits of the cards used. Any pattern or words may be printed, drawn, or stamped on the cards.

An indefinitely long series may be presented, because the hopper (*A*) may be refilled as it is emptied, and this may be kept up as long as the machine will run.

The stimuli may be presented at any desired constant rate (within rather wide limits) by simply altering the relative sizes of the pulleys on the motor and the gear reducer—an adjustment taking only a few minutes.

The time of exposure of a single card may be altered independently of the rate of presentation, because the exposure time depends upon the length of time the light is on in box (*M*) and that may be controlled by varying the size of cam used to operate the light contact.

The order in which the stimuli are presented may be easily changed by rearranging the cards before stacking them into the machine.

After presentation the cards are restacked in holder (*V*) in the same order as they were in hopper (*A*) above.

Also the contacts operated by the machine itself, or by the falling cards, may be used to determine the arrangement of warnings, signals, or punishments desired for any response to a given stimulus.

The machine is quite compact, the measurements over all being 12″ wide, 24″ long, and 48″ high. By detaching the top hopper (*A*) and the bottom holder (*V*), the height of the apparatus is reduced from 48″ to 17″.

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ROBERT S. WOODWORTH. *Contemporary Schools of Psychology.* New York: Ronald Press, 1931. Pp. vi+232. $2.50.

While this book cannot be considered an important original contribution to psychology, it does present a clear, concise, and exceptionally interesting account of the basic principles, methods, and results of the outstanding contemporary schools. Dr. Woodworth's method has been to introduce the school by means of a discussion of its chief "predisposing and exciting causes," to present a discussion of its main tenets, to discuss typical pieces of research, and, finally, to evaluate its contribution to the science as a whole. In most cases his object has been to expound rather than to criticize. Consequently, one need not expect to discover in this book a thoroughgoing critique of the points of view presented.

The introductory chapter deals with the development of psychology from Descartes to the present day. Obviously this discussion is a mere sketch. Introspection and existentialism receive a somewhat lengthy discussion. While introspection is considered the sole property of no one school of psychology, the author feels that the existentialists have expanded its use to the exclusion of performance, which is the main interest of the objectivists. The existential viewpoint, while primarily Titchnerian, originated in the attempts of Wundt, Mach, and Avenarius to distinguish psychology and physics. This distinction led to the study of experiences "as *existences*, as facts deserving of description, analysis and classification just on their own account . . . . existential psychology was to be interested in the individual as an experiencer and not as a performer." Existential psychology today is, for Dr. Woodworth, but an "ideal or a program for investigation."

Behaviorism, unlike existentialism, is interested in performance, to the exclusion of experience. It "did not, like Minerva, spring full-grown from the head of Jove, but had developed gradually in the womb of psychology . . . . the outburst of behaviorism in 1912-14 had its predisposing and its

exciting causes, such as the psychiatrist distinguishes in tracing the origin of an attack of insanity." These causes were, respectively, the already existing objective work in the general field of psychology and the ambiguous position of the various psychologists interested in performance who, at the hands of the introspectionists, were considered outside the pale of psychology. The objective movement which culminated in Watsonian behaviorism is traced back to Ebbinghaus, Galton, Cattell, Pavlov, and that group of psychologists and near-psychologists who were interested primarily in how an organism behaves. Even McDougall, Pillsbury, and the functionalists made their contributions by stressing performance. The chief significance of the behavioristic movement, according to Dr. Woodworth, is that it has forced the psychologists who are interested in performance to speak in terms of performance instead of in terms of "modifications of consciousness." He also stresses the fact that the movement is exerting a profound influence on sociology, ethics, and religion.

The work of Franz and Lashley on brain functions, the author feels, is bridging the gap which, for a while, seemed to divide the behaviorists from the Gestalt group. He points out that the Gestalt movement is definitely a revolt against the doctrine of elements characteristic of associationism, existentialism and behaviorism. While its modern exponents have given the greatest impetus to Gestalt, its inception is to be found in the work of Ehrenfels and the *Gestaltqualität* group. The chief significance of the school, according to Dr. Woodworth, is its experimental attack upon problems neglected by the other schools, principally perception and insight.

The chapter on psychoanalysis and related schools is one of the best in the whole book. In a clear and concise manner Dr. Woodworth wades through the usually bewildering maze of unconscious wishes, symbols, ids, egos, and super-egos. He emerges with what appears to him to be the three outstanding tenets of the Freudian psychoanalysts, namely, universal motivation, permanence of particular desires, and individual polarity. "While . . . . the superstructure of his [Freud's] psychological system may be summed up in the words 'repressed infantile sexuality' the foundation might be suggested by 'polarized wishful entities.'" Dr. Woodworth feels that Freud's chief contribution has probably been in his practical contact with, and his rational consideration of, sex in its relation to personality problems, rather than in his theoretical pronouncements. Adler and Jung, their relation to the Freudian school, and their main contributions are clearly presented.

Purposivism or hormic psychology receives a short consideration. The importance in the field of social science of Dr. McDougall's "Social psychology" and the influence of the concept of purpose on the various schools of psychology are stressed. In this discussion Dr. Woodworth gives due recognition to the reaction against instinct and purpose on the part of the larger group of psychologists.

The final chapter in the book is entitled "The middle of the road." Here we find a discussion of the eclectics. The great majority of psychologists, and many of the important ones, according to the author, are not to be found in any particular school of psychology. They are working in the laboratories and making use of contributions that any of the schools may contribute to their problems. In conclusion, he says, "Our beautiful psychology may owe its peculiar type of exciting charm to the fact that it . . . . is young. The glaciers of philosophy—of course it must be philosophy—having but recently retreated to the high mountains, have left us our cliffs and waterfalls. But erosion has already set in and is laboring to reduce the boldness of those cliffs and the exciting dash of those waterfalls. Even where El Capitan now rears his massive front we may expect in time to see but a gentle wooded slope. What a pity!—but the glaciers may come again. Meanwhile, the older, well-cultivated valley certainly has some advantages." No doubt Dr. Woodworth's mild insinuations together with the readability of his book will lure many a budding psychologist into the cultivated valley of eclecticism. For the volume throughout reads very much like a plea for eclecticism. It should prove valuable as an introduction to systematic psychology, both for the student and the general reader. Its style is equally adapted to both. An appendix of general readings and questions adds to the value of the work.

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